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THE VEGETATION OF THE SARGASSO SEA.

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(*Read April 24, 1914.*)

On September 16, 1492, Columbus encountered masses of floating seaweed in latitude 28° N. 58° W. as he was approaching the Bahama Islands. This is the first record of the existence of what is now known as the Sargasso Sea. Since that date many navigators and travellers, who have traversed that region, have described the general appearance of the sea and have attempted to ascertain its limits and to explain the source from which the floating gulf-weed was derived. Unfortunately, however, the earlier accounts were often rather vague and to some extent conflicting and even well-known scientific men, as Humboldt, have been too much inclined to call attention to the sea as one of the wonders of nature rather than to attempt to record the facts about it accurately. Humboldt, for instance, described the Sargasso Sea as an area six times as large as Germany covered with a growth of a single species of seaweed which he regarded as very remarkable considering the small size of the land areas covered by the growth of a single species of plant. Although Humboldt's account is in a sense true, the impression that those who read his account receive is misleading.

The account given by Alexander Agassiz in 1888 was less sensational. He says:

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"The Sargasso Sea of the North Atlantic covers a rather indefinite area between 22° and 36° N. and according to the statements of the older navigators, the amount of Sargassum to be met with varies from occasional patches to masses large enough to impede the progress of sailing vessels. The Sargassum probably changes its position constantly, according to the seasons, the currents, and the direction of the wind; but within the area bounded by the Gulf Stream on the West, the equatorial current on the South and the return current from the Azores and Canaries the Sargassum has always been found in larger or smaller quantities."

At the present day we have a definite knowledge of the ocean currents and the prevailing winds of the Atlantic which are important factors in the distribution of the gulf-weed. My personal experience, which has been confined to that part of the Ocean lying between New York and Bermuda, agrees with that of most recent travellers who have traversed the Sargasso Sea in various directions and it may be said that the gulf-weed occurs in scattered patches which are usually from fifty to, at the most, a few hundred feet in diameter. It appears to be certain that in no place is the Sea covered by the gulf-weed in continuous masses miles in extent and it is not often the case that the patches extend over a space as large as an acre. Their long diameter is usually in the direction of the wind and their frequency varies very much according to circumstances. Whether they are ever so dense as actually to impede modern sailing vessels seems to me doubtful, and it must be said that those who make the statement that the progress of vessels may be impeded by the gulf-weed usually preface their remarks by saying 'according to older navigators' and do not depend on their own observations.

If at the present day we have a good topographical knowledge of the Sargasso Sea, the question as to the particular species to which the gulf-weed belongs still presents several perplexing problems and, as to the origin of the gulf-weed, as recently as 1907 Sauvageau wrote: "les causes de sa formation ne sont guère mieux connues que lors du premier voyage de Christophe Colomb." Sargassum, to which the gulf-weed, *Sargassum bacciferum*,[1] belongs is

[1] The name *Sargassum bacciferum* is used here since it has been the name most commonly used to designate the gulf-weed. Boergesen in his paper, "The species of *Sargassum* found along the coasts of the Danish West Indies with Remarks upon the Floating Forms of the Sargasso Sea," Copenhagen, 1914, states fully the reasons for preferring the name *Sargassum natans* (L.) on the ground of priority.

a large genus, the largest of all the genera of the larger brown seaweeds, and includes mainly species which inhabit the tropics, or more accurately between about 42° N. and 42° S. latitude. They flourish just below low water mark but do not grow in deep water. They are attached to the substratum by a hold-fast and grow not infrequently to be three feet long, with a branching, slender stem bearing leaves with small, stalked air-bladders near their bases. The fruit, the spores, are in cavities on special branches. The genus is a difficult one for the systematic botanist because, to be sure of a species, one should have not only the stem with its leaves but also the base and the fruit and in many cases species have been described from fragments only. Furthermore the individuals of most species vary very much so that, without a study of a set of living specimens, an algologist might be pardoned for believing that he had not one but several species before him, judging by herbarium specimens only. The points I have just mentioned must be borne in mind in what I have to say about the gulf-weed. It remains for us to consider the two questions: What is the gulf-weed and where does it come from?

All observations agree that the masses of floating gulf-weed consist in far the greater part of the single species, called *Sargassum bacciferum*. If, however, we examine more closely the traditional gulf-weed we find that although it has the characteristic leaves and bladders of the genus, it has no remains of a basal attachment and no fruit except in certain very rare and not well authenticated cases. Some believing that, if not impossible, it is certainly very improbable that any species could continue to flourish indefinitely like the gulf-weed without at some time fruiting and, furthermore, seeing a certain resemblance of the leaves and bladders to those of certain species of Sargassum growing attached in the West Indies and on the Florida coast, have advanced the opinion that the floating form called gulf-weed consists of branches broken from the attached forms and carried by the gulf stream to the different parts of the Sargasso Sea. Others maintain that this is not the only case of a plant living and flourishing without producing fruit, and that, since up to the present time, no one has found the *Sargassum bacciferum* attached and fruiting, we are forced to believe that it is a distinct but always sterile species and not a form of any other attached

species. This latter opinion is the one held by most recent writers.

The question is not as simple as it seems at first sight. It may be asked whether *Sargassum bacciferum* occurs in other places than the Sargasso Sea and its immediate vicinity. What has been considered to be this species has been reported to occur in New Zealand, Australia, Java and various places in the Pacific and Indian Oceans as well as Valparaiso but only scattered specimens have been found and there is no evidence whatever that there is more than one Sargasso Sea in the world and it may be questioned whether all the specimens supposed to be *S. bacciferum* from other regions are really the same as the Atlantic form. I have a specimen marked New Zealand which seems to be the real gulf-weed but the data on the label are scanty and I do not feel sure that the locality is correctly given. Von Marten's theory that the gulf-weed originated in the Indian Ocean and was carried by currents round the Cape of Good Hope to the Sargasso Sea has nothing to support it, nor can the theory of Ed. Forbes that the floating gulf-weed is the survival of Sargassum growing on the submerged Atlantis be seriously considered.

As a waif, or straggler, the gulf-weed is occasionally deposited on the shores of northwestern Europe but in Great Britain, at least, it must be very rare for in his Phycologia Britannica Harvey was obliged to draw his figure of *L. bacciferum* from an American, not a British specimen. On the east American coast specimens of the gulf-weed are very rare. The only specimen which I have is a fragment washed ashore at Bath, Long Island. Some years ago I was told by a sea captain that there was a bank of gulf-weed off Nantucket but I have been unable to obtain any confirmation of this statement. Even if there is such a bank, the chances are that it is composed of *S. filipendula,* which is very abundant on the adjacent shore of Cape Cod.

As has been said, by far the greater part of the gulf-weed masses is composed of *S. bacciferum.* That it is exclusively so is not true. Agardh states that *S. Hystrix* is found with *S. bacciferum* and recently Boergesen has reported the same species near the Danish West Indies; *S. vulgare,* a very common attached species of the West Indies has also been found with the gulf-weed. The mixture

of the two species does not appear to be common in the Sargasso Sea itself but, as one approaches the land, the floating *S. vulgare* mixed with *S. bacciferum* is common and one finds both common even on the surface of landlocked waters like Harrington Sound, Bermuda.

A very interesting case is that of the mixture of a species of Cystoseira and gulf-weed collected by Professor F. H. Storer on a voyage from the Cape of Good Hope to New York. The exact position cannot be stated but according to information given by Professor Storer it was approximately 10° N. by 40° to 45° W. One gathering only was made and from it was obtained the specimens of *S. bacciferum* distributed in the "Algæ Americæ Borealis" of Farlow, Anderson and Eaton. This set has been seen by all the well known algologists of the world and, as no one has questioned the determination, it may be supposed to be correct. The Cystoseira was entangled in the Sargassum. The species of Cystoseira are complicated and not easy to name and the specimens in question were not in fruit. As far as I could tell, the species appeared to be very near *C. crinita* Bory, a Mediterranean species. Specimens have been examined by Sauvageau, the expert student of the genus, whose opinion is that in spite of certain points in common with *C. crinita* he would not venture to assert that they belong to that species. The interesting fact, however, is that, whether *C. crinita* or not, it must have come from the southeastern shore of Europe or of Northern Africa including the Atlantic islands since the species of Cystoseira abound in that region and, with one exception, none are found on the east coast of North America. *C. Myrica* is a rare species of Florida and the Bahamas and is quite different from the floating Cystoseira. As far as could be told from the material collected by Professor Storer, the Cystoseira in spite of its long journey was in as good a condition as the gulf-weed with which it was found. This is instructive as showing how far specimens can be transported by currents without perceptible injury.

In conclusion, in the limited time at our disposal, I shall show you a few slides of the gulf-weed and related species to illustrate more clearly some of the points I have mentioned. Everything considered it seems to me that in the present state of our knowledge we

are not as yet warranted in assuming that the floating gulf-weed could not have been derived originally from some fixed, fruiting form. Certainly we do not at present know from what species it might have been derived but, until the distribution of the Sargassa on the eastern coast of America and the West Indies is better known and the characteristics and variations of the various described species have been more thoroughly studied, the question of the origin of the gulf-weed seems to me to be still open.

THE KINETIC SYSTEM.

By GEORGE W. CRILE, M.D.

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In this paper I formulate a theory which I hope will harmonize a large number of clinical and experimental data, supply an interpretation of certain diseases, and show by what means many diverse causes produce the same end effects.

Even should the theory prove ultimately to be true, it will meantime doubtless be subjected to many alterations. The specialized laboratory worker will at first fail to see the broader clinical view, and the trained clinician may hesitate to accept the laboratory findings. Our viewpoint has been gained from a consideration of both lines of evidence on rather a large scale.

The responsibility for the kinetic theory is assumed by myself, while the responsibility for the experimental data is shared fully by my associates, Dr. J. B. Austin, Dr. F. W. Hitchings, Dr. H. G. Sloan and Dr. M. L. Menten.

Introduction.

The self-preservation of man and kindred animals is affected through mechanisms which transform latent energy into kinetic energy to accomplish adaptive ends. Man appropriates from environment the energy he requires in the form of crude food which is refined by the digestive system; oxygen is taken to the blood and carbon dioxid is taken from the blood by the respiratory system; to and from the myriads of working cells of the body, food and oxygen and waste are carried by the circulatory system; the body is cleared of waste by the urinary system; procreation is accomplished through the genital system; but none of these systems are evolved primarily for the purpose of transforming potential energy into kinetic energy for specific ends. Each system transforms such amounts of potential into kinetic energy as are required to perform its specific work; but no one of them transforms latent into kinetic energy for the

purposes of escaping, of fighting, of pursuing; or for combating infection. The stomach, the kidneys, the lungs, the heart strike no physical blow—their rôle is to do certain work to the end that the blow may be struck by another system evolved for that purpose. I propose to offer evidence that there is in the body a system evolved primarily for the transformation of latent energy into motion and into heat. This system I propose to designate the Kinetic System.

The kinetic system does not directly circulate the blood, nor does it exchange oxygen and carbon dioxid; nor does it perform the functions of digestion, urinary elimination and procreation; but though the kinetic system does not directly perform these functions, it does play indirectly an important rôle in each, just as the kinetic system itself is aided indirectly by the other systems.

The principal organs which comprise the kinetic system are the brain, the thyroid, the suprarenals, the liver and the muscles. The brain is the great central battery which drives the body; the thyroid governs the conditions favoring tissue oxidation; the suprarenals govern immediate oxidation processes; the liver fabricates and stores glycogen; and the muscles are the great converters of latent energy into heat and motion.

Adrenalin alone, thyroid extract alone, brain activity alone, and muscular activity alone are capable of causing the body temperature to rise above the normal. The functional activity of no other gland of the body alone, and the secretion of no other gland alone can cause a comparable rise in body temperature—that is, increased functional activity; and no active principle derived from the kidney, the liver, the stomach, the pancreas, the hypophysis, the parathyroid, the spleen, the intestines, the thymus, the lymphatic glands or the bones can, *per se*, cause a rise in the general body temperature comparable to the rise that may be caused by the activity of the brain or the muscles, or by the injection of adrenalin or thyroid extract. Then, too, when the brain, the thyroid, the suprarenals, the liver or the muscles are eliminated, the power of the body to convert latent into kinetic energy is impaired or lost. I shall offer evidence tending to show that an excess of either internal or external environmental stimuli may modify one or more organs of the kinetic system, and that this modification may cause certain diseases. For example,—

alterations in the efficiency of the cerebral link may yield neurasthenia, mania, dementia; of the thyroid link,—Graves' disease, myxedema; of the suprarenal link,—Addison's disease, cardiovascular disease.

This introduction may serve to give the line of our argument. We shall now consider briefly certain salient facts which relate to the conversion of latent energy into kinetic energy as an adaptive reaction. The amount of experimental data is so large that they will later be published in a monograph.

The amount of latent energy which may be converted into kinetic energy for adaptive ends varies in different species, in individuals of the same species, in the same individual in different seasons; in the life cycle of growth, reproduction and decay; in the waking and sleeping hours; in disease and in activity. We shall here consider briefly the reasons for some of those variations and the mechanism which makes them possible.

Biologic Consideration of the Adaptive Variation in Amounts of Energy Stored in Various Animals.

Energy is appropriated from the physical forces of nature that constitute the environment. This energy is stored in the body in quantities in excess of the needs of the moment. In some animals this excess storage is greater than in other animals. Those animals whose self-preservation is dependent on purely mechanical or chemical means of defense, such animals as crustaceans, porcupines, skunks or cobras, have a relatively small amount of convertible (adaptive) energy stored in their bodies. On the contrary, the more an animal is dependent on its muscular activity for self-preservation the more surplus available (adaptive) energy there is stored in its body. It may be true that all animals have approximately an equal amount per kilo of chemical energy—but certainly they have not an equal amount stored in a form which is available for immediate conversion for adaptive ends.

Adaptive Variation in the Rate of Energy Discharge.

What chance for survival would a skunk have without odor; a cobra without venom; a turtle without carapace; or a porcupine

shorn of its barbs, in an environment of powerful and hostile carnivora? And yet in such a hostile environment many unprotected animals survive by their muscular power of flight alone. It is evident that the provision for the storage of "adaptive" energy is not the only evolved characteristic which relates to the energy of the body. The more the self-preservation of the animal depends on motor activity, the greater is the range of variation in the rate of discharge of energy. The rate of energy discharge is especially high in animals evolved along the line of hunter and hunted, such as the carnivora and the herbivora of the great plains.

Influences That Cause Variation in the Rate of Output of Energy in the Individual.

Not only is there a variation in the rate of output of energy among various species of animals, but one finds also variations in the rate of output of energy among individuals of the same species. If our thesis that men and animals are mechanisms responding to environmental stimuli is correct, and further, if the speed of energy output is due to changes in the activating organs as a result of adaptive stimulation, then we should expect to find physical changes in the activating glands during the cycles of increased activation. What are the facts? We know that most animals have breeding seasons evolved as adaptations to the food supply and weather. Hence there is in most animals a mating season in advance of the season of maximum food supply so that the young may appear at the period when food is most abundant. In the springtime most birds and mammals mate, and in the springtime at least one of the great activating glands is enlarged—the thyroid in animals and in man shows seasonal enlargement. The effect of the increased activity is seen in the song, the courting, the fighting, in the quickened pulse and in a slightly raised temperature. Even more activation than that connected with the season is seen in the physical act of mating—when the thyroid is known to enlarge materially, though this increased thyroid activity, as we shall show later, is probably no greater than the increased activity of other activating glands. In the mating season the kinetic activity is speeded up; in short, there exists a state—a fleeting state—of mild Graves' disease; in the early stages of Graves' disease, before

the destructive phenomena are felt, the kinetic speed is high and life is on a sensuous edge. Not only is there a seasonal rhythm to the rate of flow of energy, but there is a diurnal variation, the ebb is at night, and the full tide in the daytime. This observation is verified by experiments which show that certain organs in the kinetic chain are histologically exhausted, the depleted cells being for the most part restored by sleep.

We have seen that there are variations in speed in different species, and that in the same species speed varies with the season of the year and with the time of day. In addition there are variations also in the rate of discharge of energy in the various cycles of the life of the individual. The young are evolved at high speed for growth, so that as soon as possible they may attain to their own power of self-defense; they must adapt themselves to innumerable bacteria; to food, and to all the elements in their external environment. Against their gross enemies the young are measurably protected by their parents; but the parents—except to a limited extent in the case of man—are unable to assist in the protection of the young against infectious disease.

The cycle of greatest kinetic energy for physiologic ends is the period of reproduction. In the female especially there is a cycle of increased activity just prior to her development into the procreative state. During this time secondary sexual characters are developed—the pelvis expands, the ovaries and the uterus grow rapidly, the mammary glands develop. Again in this period of increasing speed in the expenditure of energy we find the thyroid, the suprarenal and the hypophysis also in rapid growth. Without the normal development of the ovary, the thyroid and the hypophysis, neither the male nor the female can develop the secondary sexual characters, nor do they develop sexual desire nor show seasonal cycles of activity, nor can they procreate. The secondary sexual characters—sexual desire, fertility—may be developed at will—for example, by feeding thyroid products from alien species to the individual deprived of the thyroid.

At the close of the childbearing period there is a permanent diminution of the speed of energy discharge, for energy is no longer needed as it was for the self-preservation of the offspring before

adolescence, and for the propagation of the species during the procreative period. Unless other factors intervene this reduction in speed is progressive until senescent death. The diminished size of the thyroid of the aged bears testimony to the part the activating organs bear in the general decline.

We have now referred to variations in the rate of discharge of energy in different species; in individuals of the same species; in cycles in the same individual—such as the seasons of food supply; the periods of wakefulness and of sleep; the procreative period—and we have spoken of those variations caused artificially by thyroid feeding.

Thus far we have referred to the conversion for adaptive purposes of latent into kinetic energy in muscular and in procreative action. We shall now consider the conversion of latent into kinetic energy in the production of heat,[1] and endeavor to answer the questions which arise at once:—Is there one mechanism for the conversion of latent energy into heat and another mechanism for its conversion into muscular action? What is the adaptive advantage of fever in infection?

The Purpose and the Mechanism of Heat Production in Infections.

Vaughn has shown that the presence in the body of any alien protein causes an increased production of heat, and that there is no difference between the production of fever by foreign protein and by infections. Before the day of the hypodermic needle and of experimental medicine, the foreign proteins found in the body outside the alimentary tract were brought in by invading microörganisms. Such organisms interfered with and destroyed the host. The body, therefore, was forced to evolve a means of protection against these hostile organisms. The increased metabolism and fever in infection might operate as a protection in two ways: the increased fever by interfering with bacterial growth, and the increased metabolism by breaking up the bacteria. Bacteriologists have taught us that bacteria grow best at the normal temperature of the body, hence fever

[1] We use the terms heat and muscular action in the popular sense, though physicists use them to designate one and the same kind of energy.

would interfere with bacterial growth. With each rise of one degree centigrade the chemical activity of the body is increased ten per cent. In acute infections there is aversion to food and frequently there is vomiting. In fever, then, we have a diminished intake of energy, but an increased output of energy—hence the available potential energy in the body is rapidly consumed. This may be an adaptation for the purpose of breaking up the foreign protein molecules composing the bacteria. Thus the body may be purified by a chemical combustion so furious that frequently the host itself is destroyed. The problem of immunity is not considered here.

As to the mechanism which produces fever, we postulate that it is the same mechanism as that which produces muscular activity. Muscular activity is produced by the conversion of latent energy into motion, and fever is produced largely in the muscles by the conversion of latent energy into heat. We should, therefore, find similar changes in the brain, the suprarenals, the thyroid, and the liver, whatever may be the purpose of the conversion of energy—whether for running, for fighting, for the expression of emotion, or for combating infection.

We shall first present experimental and clinical evidence which tends to show what part is played by the brain in the production of both muscular and febrile action, and later we shall discuss the parts played by the suprarenals, the thyroid, and the liver.

Histologic Changes in the Brain-Cells in Relation to the Maintenance of Consciousness and to the Production of the Emotions, Muscular Activity and Fever.

We have studied the brain-cells in human cases of fever, and in animals after prolonged insomnia; after the injection of the toxins of gonococci, of streptococci, of staphylococci, and of colon, tetanus, diphtheria and typhoid bacilli; and after the injection of foreign proteins, of indol and skatol, of leucin and of peptones. We have studied the brains of animals which had been activated in varying degrees up to the point of complete exhaustion by running, by fighting, by rage and fear, by physical injury and by the injection of strychnia. We have studied the brains of salmon at the mouth of the Columbia River and at its headwater; the brains of electric fish,

the storage batteries of which had been partially discharged, and of those the batteries of which had been completely discharged; the brains of woodchucks in hibernation and after fighting; the brains of humans who had died from anemia resulting from hemorrhage, from acidosis, from eclampsia, from cancer, and from other chronic diseases. We have studied also the brains of animals after the excision of the suprarenals, of the pancreas, and of the liver.

In every instance the loss of vitality—that is, the loss of the normal power to convert potential into kinetic energy—was accompanied by physical changes in the brain-cells. The converse was also true—that is, the brain-cells of animals with normal vital power showed no histologic changes. The changes in the brain-cells were identical whatever the cause. The crucial question then becomes: Are these constant changes in the brain-cells the result of work done by the brain-cells in running, in fighting, in emotion, in fever? In other words, does the brain perform a definite rôle in the conversion of latent energy into fever or into muscular action; or are the brain-cell changes caused by the chemical products of metabolism? Happily this crucial question was definitely answered by the following experiment: The circulations of two dogs were crossed in such a manner that the circulation of the head of one dog was anastomosed with the circulation of the body of another dog and vice versa. A cord encircled the neck of each so firmly that the anastomosing circulation was blocked. If the brain-cell changes were due to the metabolic products, then when the body of dog "A" was injured, the brain of dog "A" would be normal and the brain of dog "B" would show changes. Our experiments showed brain-cell changes in the brain of the dog injured and no changes in the brain of the uninjured dog.

The injection of adrenalin causes striking brain-cell changes—first, a hyperchromatism, then a chromatolysis. Now if adrenalin caused these changes merely as a metabolic phenomenon and not as a "work" phenomenon, then the injection of adrenalin into the carotid artery of a crossed circulation dog would cause no change in its circulation and its respiration, since the brain thus injected is in exclusive vascular connection with the body of another dog. In our experiment the blood-pressures of both dogs were recorded on a drum when adrenalin was injected into the common carotid. The adre-

nalin caused a rise in blood-pressure, an increase in the force of cardiac contraction, increase in respiration, and a characteristic adrenalin rise in the blood-pressure of both dogs. The rise was seen first in the dog whose brain alone received adrenalin and about a minute later in the dog whose body alone received adrenalin. Histologic examinations of the brains of both dogs showed marked hyperchromatism in the brain receiving adrenalin, while the brain receiving no adrenalin showed no change. Here is a clear-cut observation on the action of adrenalin on the brain—and both the functional and the histological tests showed that adrenalin causes increased brain action. The significance of this affinity of the brain for adrenalin begins to be seen when I call attention to the following striking facts:

1. Adrenalin alone causes hyperchromatism followed by chromatolysis, and in overdosage causes the destruction of some brain-cells.

2. When the suprarenal glands are both excised and no other factor is introduced, the Nissl substance progressively disappears from the brain-cells until death. This far-reaching point will be taken up later.

Here our purpose is to discuss the cause of the brain-cell changes. We have seen that in crossed brain and body circulation trauma cause changes in the cells of the brain which is disconnected from the traumatized body by its circulation, but which is connected with the traumatized body by the nervous system. We have seen that adrenalin causes activation of the body connected with its brain by the nervous system, and histologic changes in the brain acted on directly by the adrenalin, but we found no brain-cell changes in the other brain through which the products of metabolism have circulated.

In the foregoing we find direct evidence that the brain-cell changes are not due to the products of metabolism. We shall now present evidence to show that the brain-cell changes are "work" changes. What work? We postulate that it is the work by which the energy stored in the brain-cells is converted into electricity or some other form of transmissible energy which then activates certain glands and muscles, thus converting latent energy into heat and motion. It has chanced that certain other studies have given an analogous and convincing proof of this postulate. In the electric fish a part of the muscular mechanism is replaced by a specialized structure for storing

and discharging electricity. We found "work" changes in the brain-cells of electric fish after all their electricity had been rapidly discharged. We found further that electric fish could not discharge their electricity when under anesthesia, and clinically we know that under deep morphia narcosis, and under anesthesia, the production both of heat and of muscular action is hindered. The action of morphia in lessening fever production is probably the result of its depressing influence on the brain-cells, because of which a diminished amount of their potential energy is converted into electricity and a diminished electric discharge from the brain to the muscles should diminish heat production proportionally. We found by experiment that under deep morphinization brain-cell changes due to toxins could be largely prevented; in human patients deep morphinization diminishes the production of muscular action and of fever, and as we shall see later conserves life when it is threatened by acute infections. The contribution of the brain-cells to the production of heat is either the result of the direct conversion of their stored energy into heat, or of the conversion of their latent energy into electricity or a similar force, which in turn causes certain glands and muscles to convert latent energy into heat.

A further support to the postulate that the brain-cells contribute to the production of fever by sending impulses to the muscles is found in the effect of muscular exertion, or of other forms of motor stimulation in the presence of a fever-producing infection. Under such circumstances muscular exertion causes additional fever, and causes also added but identical changes in the brain-cells. Thyroid extract and iodin have the same effect as muscular exertion and infection in the production of fever and the production of brain-cell changes. All of this evidence is a strong argument in favor of the theory that certain constituents of the brain-cells are consumed in the work performed by the brain in the production of fever.

That the stimulation of the brain-cells without gross activity of the skeletal muscles and without infection can produce heat is shown as follows:

(*a*) Fever is produced when animals are subjected to fear without any consequent exertion of the skeletal muscles.

(*b*) The temperature of the anxious friends of patients will rise while they await the outcome of an operation.

(*c*) The temperature of patients will rise as a result of the mere anticipation of a surgical operation.

(*d*) There are innumerable clinical observations as to the effect of emotional excitation on the temperature of patients. A rise of a degree or more is a common result of a visit from a tactless friend. There is a traditional Sunday increase of temperature in hospital wards. Now the visitor does not bring and administer more infection to the patient to cause this rise, and the rise of temperature occurs even if the patient does not make the least muscular exertion as a result of the visit. I observed an average increase of one and one eighth degrees of temperature in a ward of fifteen children as a result of a Fourth of July celebration.

Is the contribution of the brain to the production of heat due to the conversion of latent energy directly into heat, or does the brain produce heat principally by converting its latent energy into electricity or some similar form of transmissible energy which through nerve connections stimulates other organs and tissues, which in turn convert their stores of latent energy into heat?

According to Starling, when the connection between the brain and the muscles of an animal is severed by curare, by anesthetics, by the division of the cord and nerves, then the heat-producing power of the animal so modified is on a level with that of cold-blooded animals. With cold the temperature falls, with heat it rises. Such an animal has no more control over the conversion of latent energy into heat than it has over the conversion of latent energy into motion.

Electric stimulation done over a period of time causes brain-cell changes, and electric stimulation of muscles causes a rise in temperature.

Summary.

In our crossed circulation experiments we found that the brain-cell changes were not due to waste products or to metabolic poisons. We found that in the production both of muscular action and of fever there were brain-cell changes which showed a quantitative relation to the temperature changes or to the muscular

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work done. We observed that under deep morphinization the febrile response or the muscular work done was either diminished or eliminated and that the brain-cell changes were correspondingly diminished or eliminated. We found also that brain-cell changes and muscular work followed electric stimulation alone. I conclude, therefore, that the brain-cell changes are work changes.

We shall next consider other organs of the kinetic system in their relation to muscular activity, to emotion, to consciousness, to sleep, to hibernation, and to heat production.

The Suprarenal Gland.

In our extensive study of the brain in its relation to the production of energy and consequent exhaustion caused by fear and rage; by the injection of foreign proteins, of bacterial toxins and of strychnin; by anaphylaxis; by the injection of thyroid extract, of adrenalin, and of morphin; we found that with the exception of morphin each of these agents produced identical changes in the brain-cells. As we believed that the suprarenal glands were intimately associated with the brain in its activities, we concluded that the suprarenals also must have been affected by each of these agents. To prove this relation, we administered the above-mentioned stimuli to animals and studied their effects upon the suprarenal glands by functional, histological and surgical methods, the functional tests being made by Cannon's method.

Functional Study of the Suprarenal Glands by Cannon's Method.

Our method of applying the Cannon test for adrenalin was as follows: (*a*) The blood of the animal's was tested before the application of the stimulus. If this test was negative, then (*b*) the stimulus was applied and the blood again tested. If this test was negative, a small amount of adrenalin was added. If a positive reaction was then given, the negative result was accepted as conclusive. (*c*) If the control test was negative, then the stimulus was given. If the blood after stimulation gave a positive result for adrenalin, a second test of the same animal's blood was made twenty-five minutes or

more later. If the second test was negative, then the positive result of the first test was accepted as conclusive.

We have recorded sixty-six clear-cut experiments on dogs, which show that after fear and rage, after anaphylaxis, after injections of indol and skatol, of leucin and tyrosin, of the toxins of diphtheria and colon bacilli, of streptococci, and staphylococci, of foreign proteins and of strychnin, the Cannon test for adrenalin was positive. The test was negative after trauma under anesthesia, and after intravenous injections of thyroid extract, of thyroglobin and of the juices of various organs injected into the same animal from which the organs were taken. Placental extract gave a positive test. The test was sometimes positive after electric stimulation of the splanchnic nerves. On the other hand, if the nerve supply to the suprarenals had been previously divided, or if the suprarenals had been previously excised, then the Cannon test was negative, after the administration of each of the foregoing adequate stimuli. Blood taken directly from the suprarenal vein gave a positive result, but under deep morphinization the blood from the suprarenal vein was negative, and under deep morphinization the foregoing adequate stimuli were negative.

In brief, the agencies that in our brain-cell studies were found to cause hyperchromatism followed by chromatolysis, gave positive results in the Cannon test for adrenalin. The one agent which was found to protect the brain against changes in the Nissl substance—morphin—gave a negative result in the Cannon test for adrenalin. After excision of the suprarenals, or after division of their nerve supply, all Cannon tests for adrenalin were negative.

Histologic Studies of the Suprarenal Glands.

Histologic studies of the suprarenal glands after the application of the adequate stimuli which gave positive results to the Cannon tests for adrenalin are now in progress and thus far the histologic studies corroborate the functional tests.

In hibernating woodchucks, the cells of the adrenal cortex were found to be vacuolated, and shrunken. In 100 hours of insomnia, in surgical shock, in strong fear, in exhaustion from fighting, in peptone injections, in acute infections, the suprarenal glands undergo

histological changes characteristic of exhaustion. Alkalies cause suprarenal changes, but acids do not.

We have shown that brain and suprarenal activity go hand in hand—that is, that the suprarenal secretion activates the brain, and that the brain activates the suprarenals. The fundamental question which now arises is this: Are the brain and the suprarenals interdependent? A positive answer may be given to this question, for the evidence of the dependence of the brain upon the suprarenals is as clear as is the evidence of the dependence of the suprarenals upon the brain. (1) After excision of the suprarenals, the brain-cells undergo continuous histological and functional deterioration until death. During this time the brain progressively loses its power to respond to stimuli and there is also a progressive loss of muscular power and a diminution of body temperature. (2) In our crossed circulation experiments we found that adrenalin alone could cause increased brain activity, while histologically we know that adrenalin alone causes an increase of the Nissl substance. An animal both of whose suprarenals had been excised showed no hyperchromatism in the brain-cells after the injection of strychnin, of toxins, of foreign proteins, etc. (3) When the suprarenal nerve supply was divided (Cannon-Elliott), then there was no increased suprarenal activity in response to adequate stimuli.

From these studies we are forced to conclude not only that the brain and suprarenals are interdependent, but that the brain is actually more dependent upon the suprarenals than the suprarenals upon the brain, since the brain deteriorates progressively to death without the suprarenals, while the suprarenals whose connection with the brain has been broken by the division of their nerve supply will still produce sufficient adrenalin to support life.

From the strong affinity of the brain-cells for adrenalin which was manifested in our experiments, we may strongly suspect that the Nissl substance is a volatile, extremely unstable combination of certain elements of the brain-cells and adrenalin because the suprarenal glands alone do not take the Nissl stain and the brain deprived of adrenalin does not take Nissl stain. The consumption of the Nissl substance in the brain-cells is lessened or prevented by morphin as is the output of adrenalin; and the consumption of the Nissl substance

is also lessened or prevented by nitrous oxid. But morphin does not prevent the action of adrenalin injected into the circulation, hence the control of morphin over energy expenditure is exerted directly on the brain-cells. Apparently morphin and nitrous oxid both act through this interference with oxidation in the brain. We, therefore, conclude that within a certain range of acidity of the blood adrenalin can unite with the brain-cells only through the mediation of oxygen, and that the combination of adrenalin, oxygen, and certain brain-cell constituents causes the electric discharge that produces heat and motion. In this interrelation of the brain and the suprarenals, we have what is perhaps the master key to the automatic action of the body. Through the special senses environmental stimuli reach the brain and cause it to liberate energy which in turn activates certain other organs and tissues, among which are the suprarenal glands. The increased output of adrenalin activates the brain to still greater activity, as a result of which again the entire sympathetic nervous system is further activated, as is manifested by increased heart action, more rapid respiration, raised blood-pressure, increased output of glycogen, increased power of the muscles to metabolize glucose, etc.

If this conclusion is well founded, we should find corroborative evidence in histologic changes in that great store-house of potential energy, the liver, as a result of the application of each of the adequate stimuli which produced brain-cell and suprarenal changes.

The Liver.

Prolonged insomnia, prolonged physical exertion, infections, injections of toxins, and of strychnin, rage and fear, physical injury under anesthesia, in fact all of the adequate stimuli which affected the brain and the suprarenals, produced constant and identical histologic changes in the liver—the cells stained poorly, the cytoplasm was vacuolated, the nuclei were crenated, the cell membranes were irregular, the most marked changes occurring in the cells of the periphery of the lobules. In prolonged insomnia the striking changes in the liver were repaired by one seance of sleep.

Are the histologic changes in the liver cells due to metabolism or toxic products or are they "work" changes incident to the conversion of latent into kinetic energy? Are the brain, suprarenals and liver

interdependent? The following facts establish the answers to these queries:

1. The duration of life after excision of the liver is about the same as after adrenalectomy—approximately eighteen hours.

2. The amount of glycogen in the liver was diminished in all of the experiments showing brain-suprarenal activity; and when the histologic changes were repaired, the normal amount of glycogen was again found.

3. In crossed circulation experiments changes were found in the liver of the animal whose brain received the stimulus.

From these premises we must consider that the brain, the suprarenals, and the liver are mutually dependent on each other for the conversion of latent into kinetic energy. Each is a vital organ—each equally vital. It may be said that excision of the brain may apparently cause death in less time than excision of the liver or suprarenals, but this statement must be modified by our definition of death. If all the brain of an animal be removed by decapitation, its body may live on for at least eleven hours if its circulation be maintained by transfusion. An animal may live for weeks or months after excision of the cerebral hemispheres and the cerebellum, while an overtransfused animal may live many hours, for days, even after the destruction of the medulla. It is possible even that the brain actually is a less vital organ than either the suprarenals or the liver.

In our research to discover whether any other organs should be included with the brain, the suprarenals and the liver in this mutually interdependent relation, we hit upon an experiment which throws light upon this problem.

Groups of rabbits were gently kept awake for 100 hours by relays of students,—an experiment which steadily withdrew energy but caused not the slightest physical or emotional injury to any of them; no drug, toxin, or other agent was given to them; they were given sufficient food and drink. In brief, the internal and external environments of these animals were kept otherwise normal excepting for the gentle stimuli which ensured continued wakefulness. This protracted insomnia gradually exhausted the animals completely, some to the point of death even. Some of the survivors were killed im-

mediately after the expiration of 100 hours of wakefulness, others after varying intervals.

Histological studies were made of every tissue and organ in the body. Three organs, the brain, the suprarenals, and the liver, and these three only showed histologic changes. In these three organs the histologic changes were marked, and were almost wholly repaired by one seance of sleep. In each instance these histologic changes were identical with those seen after physical exertion, emotions, toxins, etc. It would appear, then, that these three organs take the stress of life—the brain is the battery, the suprarenals the oxidizer, and the liver the gasoline tank. The clear-cut insomnia experiment corresponds precisely with our other brain-suprarenal observations.

With these three kinetic organs we may surely associate also the "furnace," the muscles in which the energy provided by the brain, suprarenals and liver, plus oxygen, is fabricated into heat and motion.

Benedict in his monumental work on metabolism has demonstrated that in the normal state, at least, variations in the heart beat parallel variations in metabolism. He and others have shown that all energy of the body, whether evidenced by heat or by motion, is produced in the muscles. In the muscles then, we find the fourth vital link in the kinetic chain. The muscles move the body, circulate the blood, effect respiration, and govern the body temperature. They are the passive servants of the brain-suprarenal-liver syndrome.

Neither the brain, the suprarenals, the liver, nor the muscles, however, nor all of these together, have the power to change the rate of the expenditure of energy; to make possible the increased expenditure in adolescence, in pregnancy, in courting and mating, in infections. No one of these organs, nor all of them together, can act as a pacemaker or sensitizer. The brain acts immediately in response to the stimuli of the moment; the suprarenals respond instantly to the fickle brain and the effects of their actions are fleeting; the liver contains fuel only and cannot activate, and the muscles in turn act as the great furnace, in which the final transformation into available energy is made.

Another organ—the thyroid—has the special power of governing the *rate* of *discharge* of energy; in other words, the thyroid is the pace-maker. Unfortunately, the thyroid cannot be studied to

advantage either functionally or histologically, for there is as yet no available test for thyroidism in the blood as there is for adrenalin, and thyroid activity is not attended by striking histologic changes. Therefore the only laboratory studies which have been satisfactory thus far are those by which the iodin content of the thyroid has been established. Iodin is stored in the colloid lacunæ of the thyroid and in combination with certain proteins is the active agent of the thyroid.

Beebe has shown that electrical stimulation of the nerve supply of the thyroid diminishes the amount of iodin which it contains and it is known that in the hyperactive thyroid in Graves' disease the iodin content is diminished. The meagerness of laboratory studies, however, is amply compensated by the observations which the surgeon has been able to make on a vast scale—observations which are as definite as are the results of laboratory experiments.

The Thyroid.

The brain-cells and the suprarenal glands are securely concealed from the eye of the clinician, hence the changes produced in them by different causes escape his notice, but the thyroid has always been closely scrutinized by him. The clinician knows that every one of the above mentioned causes of increased brain-cell, suprarenal, liver and muscle activity may cause an increase in the activity of both the normal or the enlarged thyroid; and he knows only too well that in a given case of exophthalmic goiter, the same stimuli which excite the brain, the suprarenals, the liver, and the muscles to increased activity will also aggravate this disease.

The function of the thyroid in the kinetic chain is best evidenced, however, by its rôle in the production of fever. Fever results from the administration of thyroid extract alone in large doses. In the hyper-activity of the thyroid in exophthalmic goiter, one sees a marked tendency to fever; in severe cases there is daily fever. In fact, in Graves' disease we find displayed to an extraordinary degree an exaggeration of the whole action of the kinetic mechanism.

We have stated that in acute Graves' disease there is a tendency to the production of spontaneous fever, and that there is a magnified diurnal variation in temperature which is due to an increased output of energy in even the normal reaction producing consciousness. In

Graves' disease there is, therefore, a state of intensified consciousness, which is associated with low brain thresholds to all stimuli—both to stimuli that cause muscular action and to stimuli that cause fever. The intensity of the kinetic discharge is seen in the constant fine tremor. It is evident that the thresholds of the brain have been sensitized. In this hypersensitization we find the following strong evidence as to the identity of the various mechanisms for the production of fever. In the state of superlative sensitization which is seen in Graves' disease, we find that the stimuli that produce muscular movement, the stimuli that produce emotional phenomena and the stimuli that produce fever are as nearly as can be ascertained equally effective. Clinical evidence regarding this point is abundant, for in patients with Graves' disease we find that the three types of conversion of energy resulting from emotional stimulation, from nociceptor stimulation (pain), and from infection stimulation are, as nearly as can be judged, equally exaggerated. In the acute cases of Graves' disease the explosive conversion of latent energy into heat and motion is unexcelled by any other known normal or pathological phenomenon. Excessive thyroid secretion, as in thyrotoxicosis from functioning adenomata, and excessive thyroid feeding, cause all the phenomena of Graves' disease except the exophthalmos and the emotional facies. Ligation of arteries, division of nerve supply and excision of part of the gland may reverse the foregoing picture and restore the normal condition. The patient notes the effect on the second day and often within a week is relatively quiescent. On the contrary if there is thyroid deficiency there is the opposite state, a reptilian sluggishness.

At will, then, through diminished, normal or excessive administration of thyroid secretion, we may produce an adynamic, a normal, or an excessively dynamic state. By the thyroid influence, the brain thresholds are lowered and life becomes exquisite; without its influence the brain becomes a globe of relatively inert substance. Excessive doses of iodin alone cause most of the symptoms of Graves' disease. The active constituent of the thyroid is iodin in a special protein combination. Thus is stored in the colloidal spaces. Hence one would not expect to find changes in the cells of the thyroid gland as a result of increased activity unless it be prolonged.

We have thus far considered the normal rôles played by the brain, the suprarenals, the liver, the muscles and the thyroid in transforming latent into kinetic energy in the form of heat and motion as an adaptive response to environmental stimuli.

The argument may be strengthened, however, by the discussion of the effect of the impairment of any of these links in the kinetic chain upon the conversion of latent into kinetic energy.

Effect Upon the Output of Energy of Impaired or Lost Function of Each of the Several Links in the Kinetic Chain.

1. *The Brain: Cerebral softening.*—In cerebral softening we may find all the organs of the body comparatively healthy excepting the brain. As the brain is physically impaired it cannot normally stimulate other organs to the conversion of latent energy into heat or into motion, but on the contrary in these cases we find feeble muscular and intellectual power. I believe also we find that in patients with cerebral softening, infections such as pneumonia show a lower temperature range than in patients whose brains are normal.

2. *The Suprarenals.*—In such destructive lesions of the suprarenal glands as Addison's disease one of the cardinal symptoms is a subnormal temperature and impaired muscular power. Animals upon whom double adrenalectomy has been performed show a striking fall in temperature, muscular weakness—after adrenalectomy the animal may not be able to stand even—and progressive chromatolysis. The significance of the last will be pointed out later.

3. *The Liver.*—When the function of the liver is impaired by tumors, cirrhosis, or degeneration of the liver itself, then the entire energy of the body is correspondingly diminished. This diminution of energy is evidenced by muscular and mental weakness, by diminished response and by a gradual loss of efficiency which finally reaches the state of asthenia.

4. *The Muscles.*—It has been observed clinically that if the muscles are impaired by long disuse, or by a disease such as myasthenia gravis, then the range of production of both heat and motion is below normal.. This is in agreement with the experimental findings that anesthetics, curare, or any break in the muscle-brain connection causes diminished muscular and heat production.

5. *The Thyroid.*—In myxedema one of the cardinal symptoms is a persistently subnormal temperature and though prone to infection, subjects of myxedema show but feeble febrile response and readily succumb. This clinical observation is strikingly confirmed by laboratory observations; normal rabbits subjected to fear showed a rise in temperature of from one to three degrees while two rabbits whose thyroids had been previously removed and who had then been subjected to fright showed much less febrile response. Myxedema subjects show a loss of physical and mental energy which is proportional to the lack of thyroid. Deficiency in any of the organs of the kinetic chain causes alike loss of heat, loss of muscular and emotional action, of mental power and of the power of combating infections—the negative evidence thus strongly supports the positive. By accumulating all the evidence we believe we are justified in associating the brain, the suprarenals, the thyroid, the muscles and the liver as vital links in the kinetic chain. Other organs play a rôle undoubtedly, though a minor one. If our conclusions are sound, then in the kinetic system we should find an explanation of many diseases, and having found an explanation, we may find new methods of combating them.

Kinetic Diseases.

In the foregoing conclusions we find a simple explanation of certain diseases. When the kinetic system is driven at an overwhelming rate of speed—as by severe physical injury, by intense emotional excitation, by perforation of the intestines, by the pointing of an abcess into new territory, by the sudden onset of an infectious disease, by an overdose of strychnin, by a Marathon race, by a grilling fight, by foreign proteins, by anaphylaxis,—the result of these acute overwhelming activations of the kinetic system is clinically designated shock, and according to the cause is called traumatic shock, toxic shock, anaphylactic shock, drug shock, etc.

The essential pathology of shock is identical whatever the cause. If, however, instead of an intense overwhelming activation, the kinetic system is continuously or intermittently overstimulated through a considerable period of time, as long as each of the links in the kinetic chain takes the strain equally the result will be excessive

energy conversion, excessive work done; but usually, under stress, some one link in the chain is unable to take the strain and then the evenly balanced work of the several organs of the kinetic system is disturbed. If the brain cannot endure this strain, then neurasthenia, nerve exhaustion, or even insanity follows. If the thyroid cannot endure the strain it undergoes hyperplasia, which in turn may result in a colloid goiter or in exophthalmic goiter. If the suprarenals cannot endure the strain, cardiovascular disease may develop. If the liver cannot take the strain then death from acute acidosis may follow, or if the neutralizing effect of the liver is only partially lost, then the acidity may cause Bright's disease. Over-activation of the kinetic system may cause glycosuria and diabetes.

Identical physical and functional changes in the organs of the kinetic system may result from intense continued stimulation from any of the following causes, excessive physical labor, athletic exercise, worry or anxiety, intestinal auto-intoxication, chronic infections such as oral sepsis, tonsillitis and adenoids; chronic appendicitis, chronic cholecystitis, colitis, and skin infections; the excessive intake of protein food (foreign protein reaction); emotional strain, pregnancy, stress of business of professional life—all of which are known to be activators of the kinetic system.

From the foregoing statements we are able to understand the muscular weakness following fever; we can understand why the senile have neither muscular power nor strong febrile reaction; why long-continued infections produce pathologic changes in the organs constituting the kinetic chain; why the same pathologic changes result from various forms of activation of the kinetic system. In this hypothesis we find a reason why cardiovascular disease may be caused by chronic infection, by auto-intoxication, by overwork, or by emotional excitation. We now see that the reason why we find so much difficulty in differentiating the numerous acute infections from each other is because they play upon the same kinetic chain. Our postulate harmonizes the pathological democracy of the kinetic organs, for it explains not only why in many diseases the pathological changes in these organs are identical, but why the same changes are seen as the result of emotional strain and overwork. We can

thus understand how either emotional strain or acute or chronic infection may cause either exophthalmic goiter or cardiovascular disease; how chronic intestinal stasis with the resultant absorption of toxins may cause cardiovascular disease; neurasthenia or goiter. Here is found an explanation of the phenomena of shock, whether the shock be the result of toxins, of infection, of foreign proteins, of anaphylaxis, of psychic stimuli, or of a surgical operation with its combination of both psychical and traumatic elements.

This conception of the kinetic system has stood a crucial test by making possible the shockless surgical operation. It has offered a plausible explanation of the cause and the treatment of Graves' disease. Will this kinetic theory stand also the clinical test of controlling that protean disease bred in the midst of the stress of our present-day life? Present-day life, in which one must ever have one hand on the sword and the other on the throttle, is a constant stimulus of the kinetic system. The force of these kinetic stimuli may be lessened at the cerebral link by intelligent control—a protective control is empirically attained by many of the most successful men. The force of the kinetic stimuli may be broken at the thyroid link by dividing the nerve supply, reducing the blood supply, or by partial excision; or if the suprarenals feel the strain, the stimulating force may be broken by dividing their nerve supply, reducing the blood supply, or by partial excision. No theory is worth more than its yield in practice, but already we have the shockless operation, the surgical treatment of Graves' disease, the control of shock and the acute infection by overwhelming morphinization.

Conclusions.

To become adapted to their environment animals are transformers of energy. This adaptation to environment is made by means of a system of organs evolved for the purpose of converting potential energy into heat and motion. The principal organs and tissues of this system are the brain, the suprarenals, the thyroid, the muscles and the liver. Each is a vital link—each plays its particular rôle and one cannot compensate for the other. A change in any link

of the kinetic chain modifies proportionately the entire kinetic system, which is no stronger than its weakest link.

In this conception we find a possible explanation of many diseases —one which may point the way to new and more effective therapeutic measures than those now at our command.

CLEVELAND, O.,
April 23, 1914.

NEWER ASPECTS AND METHODS IN THE STUDY OF THE MECHANISM OF THE HEART-BEAT.

By ALFRED E. COHN, A.B., M.D.

(*Read April 23, 1914.*)

The interest now very widespread, in the physiology of the heart-beat developed from certain observations which Carlo Matteucci made some seventy-two years ago, and which he communicated in 1842 to the Academy at Paris. He established the fact that the muscle of a nerve-muscle preparation contracted if its nerve were laid across a second muscle which had been made to contract. He believed that the stimulus which the nerve received, and which it conveyed to its attached muscle was electrical in nature. Thirteen years later, in 1855, Kölliker and H. Müller widened the scope of Matteucci's observations by demonstrating in the same way that, if the nerve of a similar nerve-muscle preparation were laid across a heart, the muscle of the preparation likewise contracted, because, as in Matteucci's experiment, a current, called a current of action, was discharged from the contracting heart and was conveyed to the muscle.

These discoveries continue to be the primary subjects of experiment in contemporary studies in the mechanism of the heart-beat. The first experiments dealing with action currents were made by Marchand, Engelmann, and by Burdon-Sanderson and Page, who used a Bernstein rheotome in their investigations, but later the use of the capillary electrometer of Lippmann was introduced, especially by Marey, Waller, by Bayliss and Starling, Gotch and others. Marey in 1876 was the first to obtain permanent records of the action currents of the heart by photographing the motions of the meniscus of the mercury column of the electrometer on a moving sensitive surface. This record was a continuous curve in which could be distinguished various waves, one of which has been identified as synchronous with the contraction of the auricles, the upper chambers

of the heart, and certain others with the contraction of the ventricles, the lower pair of heart chambers. At first all records were obtained by applying electrodes directly to the surface of the heart as it lay exposed in the opened chest, but in 1889 Waller showed that one could obtain records of these currents by applying suitably constructed electrodes to the surface of the body without opening or injuring it. This discovery opened the way for studying these currents in the human subject. Waller also showed which were favorable and which unfavorable locations for placing electrodes, by varying the situation at which they were applied. From a consideration of records ob-

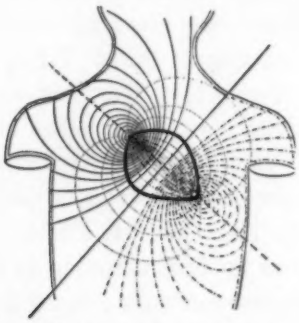


FIG. 1. After Waller. To indicate the spread of the cardiac action current through the human subject.

tained from a number of positions, now called leads or derivations, he determined the location of a plane, on the two sides of which the greatest differences in potential were developed (Fig. 1). Those locations were called favorable which yielded curves showing the largest waves, and the records were called electrocardiograms. The differences between electrocardiograms taken indirectly in this way and those taken directly from the surface of the heart are ones of contour and do not involve the important time relations of the various elements composing the curve.

With the knowledge that the heart discharged action currents, and a method for conducting these from the uninjured surface of the body to a registering instrument, the time was ripe for the construction of a galvanometer better fitted to the purposes of physiological and medical research. W. Einthoven of Leyden in 1902–6 described and built this instrument. Its completion at that time was

especially fortunate, for fresh discoveries in anatomy and physiology were almost simultaneously announced. In the interpretation of the significance of these, the galvanometric method of registration was especially valuable.

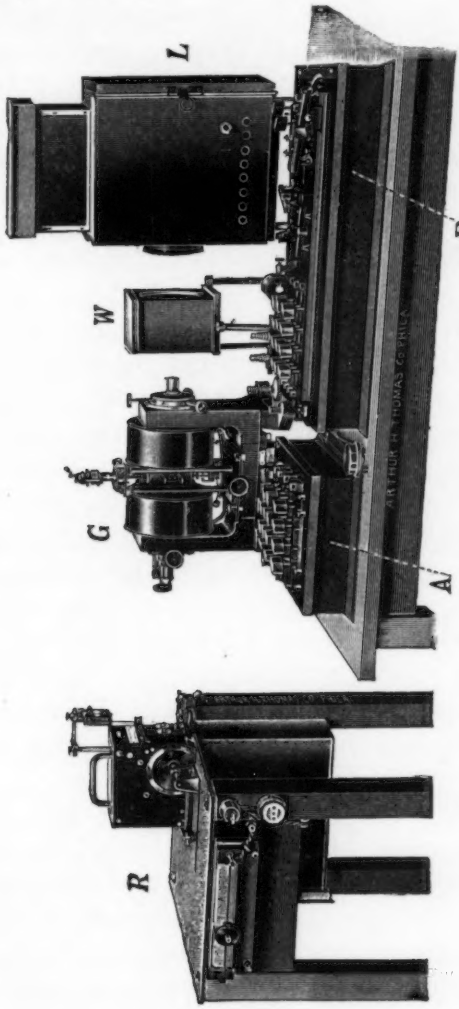


FIG. 2. String galvanometer, Edelmann's model. *R*, photographic recorder; *G*, galvanometer; *L*, arc light; *W*, water-bath; *A* and *B*, resistance and compensating boxes.

The principles Einthoven employed were in use in Deprez-D'Arsonval's instrument. From these he developed formulæ which formed the basis of the galvanometer he constructed. An instru-

ment built on similar lines for use as an ocean telegraph recorder had already been devised by Ader, but of this he was unaware at the time. It depends on the principle that a conductor suspended in a magnetic field is deflected at right angles to the lines of force when a current passes through it. The conductor chosen is usually a silvered quartz or platinum thread, 87 to 100 mm. long, 3 to 6 micra thick, having a resistance of 3,000 to 6,000 ohms It is suspended vertically between the poles of two powerful electro-magnets (Fig. 2). The thread is illuminated by the rays of an arc light which are focused on it by a system of lenses and a substage condensor. To accommodate this condensor the pole of one electro-magnet is bored. The motions of the string are magnified and projected on a recording photographic surface by a microscope held in a similar bore in the pole of the other electro-magnet. The degree of magnification may be varied according to the needs of the investigator, but, in order to maintain a degree of uniformity in the appearance and in the electrical value of curves obtained in different laboratories, certain arrangements have become conventional. These include the strength of the magnetic field, the tension of the string and its deflection time. The strength of the field depends, of course, on its construction. The tension of the string is adjusted in an appropriate manner so that a current having the value of one millivolt, when allowed to pass through it, causes a deflection of 1 cm. It has been found desirable to obtain a deflection of this extent within a definite length of time, usually 0.02 seconds or less. When the deflection time is longer, certain waves in the electrocardiogram tend to disappear.

But in order to obtain an electrocardiogram, more is necessary than to include an individual in the string circuit. For although the usual cardiac action current does not deflect the string, when it is at the prescribed tension, beyond the optical field, the skin or constant current, which is also continuously discharged from the body, and which is composed of the summed discharges from the other electrically active tissues of the organism, may do so, and it is usually sufficiently great to deflect the string far beyond the field of the microscope. This current does not show the rapid changes of potential difference that the action current from the heart does. On the

whole, its strength is uniform and changes so little within small limits of time as to render the change negligible. There is consequently no danger of confusing it with the rapid changes in electrical potential which compose the electrocardiogram. In order to keep the string in the optical field, from which the constant or skin current tends to deflect it, a system of compensation has been found necessary. This system comprises another source of current, a commutator and a series of resistances which are introduced into the string-heart circuit. The action current to be recorded, and with it the constant current, are permitted by a shunt to pass through the string in increasing amounts, so enabling one to allow a sufficient electromotive force of opposite polarity to the skin current to enter the circuit. Compensation in routine examinations becomes a simple procedure. To complete the records, a time curve and a millimeter scale are photographed on the record.

The most fruitful method of investigating the identity of the parts of the electrocardiogram has probably been that of recording synchronously on the same curve the electrocardiogram and mechanical curves representing the motions of the heart. As the result of these studies, it has been concluded that the contraction of the auricles is represented electrically by the *P* wave, the first wave in the cardiac cycle (Fig. 4), while the Waves *Q, R, S, T,* which follow it form a group in the electrocardiogram which are associated with ventricular activity. The term ventricular activity is purposely chosen, for there is as yet no uniformity of opinion in respect to designating which ventricular function it is which this complex of waves represents. Opinion is divided as to whether the complex represents the act of conduction, the state of muscular irritability, or actual contraction. It is doubtless unprofitable to analyze this discussion, and probably quite impossible to decide between these interpretations now. Of this much one can be certain, that the *QRST* complex does not occur unless the ventricles have been seen or known to contract. The significance of the individual waves of this group is also still a matter wrapped in doubt. Most writers favor the view that the wave *Q,* when present, signifies that the earliest ventricular activity has taken place near the apex of the heart, that the *R* wave represents the assumption of predominance

by the ventricular base, while *S* indicates a return of activity to the apex. The significance of the *T* wave is a hotly disputed point. Some hold it to represent the return of activity to the base of the ventricles in the later part of systole, because of an analogy which is drawn between the arterial base of the heart and the distal end of the primitive cardiac tube, which is, of course, the last segment to become affected by the wave of peristalsis which passes over it. The most important arguments against this interesting assumption have been supplied by Garten and his pupils, Clement and Erfmann. These investigators have all shown that the *T* wave occurs at the same instant of time at all points on the heart's surface, and refer its occurrence, as does also Einthoven, to a function inherent in muscular contraction. According to these authors, it represents the second wave in a current essentially diphasic. One need scarcely point out the fact that its presence simultaneously throughout the cardiac surface precludes the possibility of its occurring as the end phase of a peristaltic contraction.

Aside from the auricular representative and the group of waves representing ventricular activity, two other portions of an electrocardiographic curve must be described. The less debated of these is the portion following the *T* wave, the isoelectric period between the end of *T* and the beginning of *P*. It represents the diastole of the heart cycle,—from the end of the ventricular to the beginning of the next auricular contraction,—the rest period of the heart. The other portion is that lying between the wave *P* and the complex *QRST;* this portion also is usually isoelectric, but occasionally, as Einthoven has pointed out, its level departs from the base line. It is the period which represents the time occupied by the passage of impulses from the contracting auricles to the beginning of ventricular activity, and is called the conducting period.

We must now return to consider those other newer aspects of the study of the mechanism of the heart-beat to which I have referred in speaking of recent anatomical and physiological contributions. Before 1883 the theories held to explain coördination between the upper or auricular pair of the cardiac chambers and the lower ventricular pair consisted principally of an old notion of Haller's to the effect that the ventricles contracted in response to stimuli conveyed

to them by the act of filling, while others held and some do still that coördination between the two pairs rested on a carefully adjusted mechanism involving the passage of impulses over nervous channels. The need for theories of this sort lay in the fact that no muscular connection between the auricles and ventricles was known to exist. But in 1883, W. Gaskell convinced himself that the conduction of impulses in the heart must pass over muscular pathways, and Wooldridge and Tigerstedt contributed experiments which pointed to the

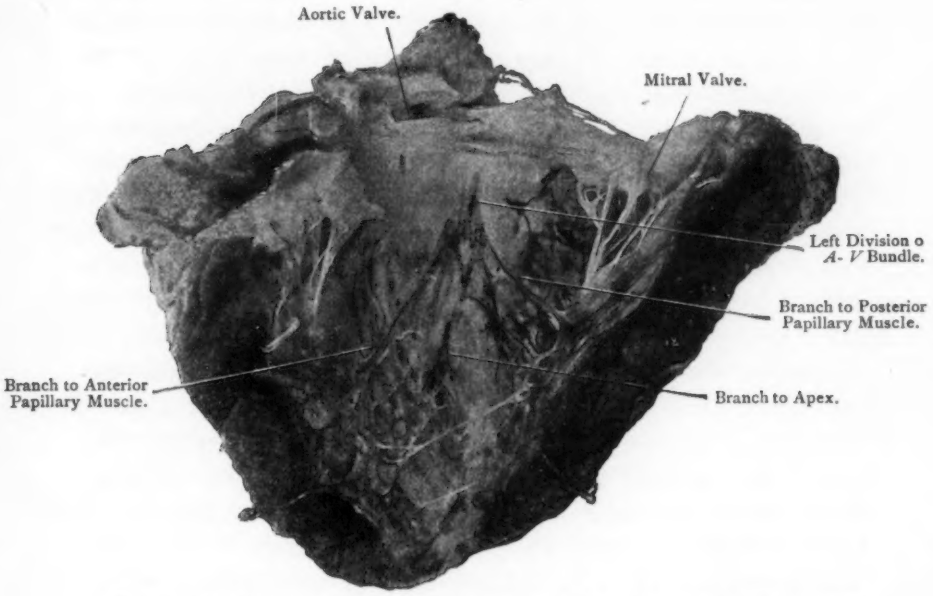


FIG. 3. Ox-heart. Injection with dilute India ink of the bursa-like spaces surrounding the left branch of the *A–V* bundle.

probability of Gaskell's contention. Ten years later (1893) Stanley Kent and His, Jr., actually saw and described a bundle of connecting muscular fibers. Since then (1893–1908) the existence of this structure, best called the auriculo-ventricular bundle, has been sufficiently confirmed. It passes from the lowest level of the auricles, divides into two and supplies a branch to each ventricle (Fig. 3). It has a peculiar muscular structure. In some species it contains large neural elements, but in man and the higher mammals only fine nerve fibrillæ

are found within it. With the establishment of the fact that the auriculo-ventricular bundle possessed this mixed structure, discussion as to whether conduction was nervous or muscular has gone out of fashion. Its essential function consists in conducting impulses and so coördinating the rhythms of the auricles and ventricles. To prove that it actually performs this function, its structure has been destroyed in experiments. In these the result anticipated was realized; auricles and ventricles continued to beat, each at a rate of its own and each in a rhythm without reference to the other. Two other facts are known about this bundle; first, that it can conduct impulses in a backward direction (from ventricles to auricles) as well as forwards; and second, that it conducts a little slower, especially at its upper terminal, than the rest of the heart muscle. We shall return to a consideration of this structure in relation to electrocardiography.

In 1906 another structure situated at the junction of the superior vena cava and the right auricular appendix was discovered by Keith and Flack. It is a small structure called the sino-auricular node. It has a sectional area of 0.3 by 0.1 cm., and attains a length of from 2.0 to 3.0 cm. Its existence has been abundantly verified. Like the conduction bundle, it also contains large and fine nerve elements. The comparative anatomy of both these structures has been traced by Ivy Mackenzie and by Külbs, while the embryology has been studied by Professor Mall. It is the discovery of these two structures, the sino-auricular node and the auriculo-ventricular system, which has added a second new and significant chapter to our store of information relating to the mechanism of the heart-beat.

The sino-auricular node has been recognized as the structure which usually initiates impulses for the contraction of the whole heart, and at the same time sets and maintains its rate. These properties have been attributed to it because excision or exclusion of the node from function reduces the rate of the heart-beat, and sometimes even stops it. Afterwards it begins gradually to contract again, but the original rate is not restored. It can usually be shown that another portion of the heart now sets the pace. Other methods have been employed to ascertain its functions; warmth applied to the site of the node accelerates the rate of the heart; cooling slows it. Attempts to alter the temperature elsewhere of the surface do not

have this effect. But more important information still is gained by means of the galvanometer. It has been demonstrated that each heart-beat begins at the node, because artificial contractions due to stimuli applied here yield curves of a shape identical with those resulting from spontaneous discharges; similar stimuli applied elsewhere fail of this identity. And finally, the law that the site at which contraction begins is primarily negative to other portions of the same strip of muscle is valid here. Lewis and Oppenheimer, Wybauw, Clement and Sulze have all been able to show that in contraction, the site of the node is primarily negative to all other portions of the auricular surface.

We have described the newly discovered structures in the mammalian heart. The function of the sinus node, in so far as it is now known, is to initiate impulses for the whole heart and to determine their rate; Lewis has aptly called it the pacemaker. The auriculoventricular bundle provides for coördination in the complicated mechanism of the heart. We must next show how, in the light of these structures, the electrocardiogram has been employed in elucidating the mechanism of the heart-beat. Althought the sinus node and the conduction bundle are very small indeed in comparison with the size of the whole heart, it is chiefly to these that the attention of investigators has been directed, while to the great mass of the organ which is charged with the real work of carrying on the circulation, very little research has been devoted. Our account of the electrophysiology of the heart must, therefore, be necessarily incomplete, and incomplete in just the direction in which one had hoped for light, —namely, in an attempt to employ electrical estimations as measures of the contractile force of the heart.

To be useful, the first demand of a method is that it give constant readings, and observation has shown that the electrocardiogram satisfies this demand, for its waves tend to remain unaltered in shape and size. When they alter, an ascertainable disturbance has in many cases been found as the cause. An electrocardiogram is, therefore, a reliable record. Its constancy is illustrated in records obtained from various classes of animals. They have certain characteristics in common, so that one can easily distinguish, for example, electrocardiograms of amphibia from those of the higher mammals. And

of the mammals, the species which have been studied, the horse, rabbit, cat, dog and man, each shows certain definite characters on the basis of which it can be recognized. Einthoven and Lewis and Gilder have studied human electrocardiograms and have defined within certain limits the usual form of curves taken from normal persons. But differentiation can go further, for the records of individuals have been found to differ widely from person to person in health, and more widely still in disease. Whatever form, either normal or abnormal, the curve assumes, this remains characteristic for the individual, for a certain length of time at all events, though the records be registered by different instruments and recorded by different investigators. An electrocardiogram may indeed attain a form so peculiarly personal that the suggestion has actually been made that it be employed to serve the purposes of identification in much the same way as the Bertillon system does. We may therefore regard the electrocardiogram as a valid and reliable record.

A number of the factors which can bring about variations from a normal curve are understood. Some of these may now be enumerated. The auricular wave, in the first place, is modified by the nature of the derivation or lead used; in this case the most favorable situations to employ are usually the two upper extremities, but under certain circumstances two points on the chest wall have been found preferable. A rare and not altogether satisfactorily established defect in the auricular mechanism is a lack of synchronicity between the contractions of the two auricles. This defect has occasionally been found to split the *P* wave. But the most significant alteration occurs when the *P* wave, instead of being directed upward, the direction which in the usual arrangement is associated with primary negativity at the site of the sinus node, is directed downward, an alteration which presumably shows that primary negativity has occurred at a lower level of the auricles. This change takes place when the sinus node is excluded from function, and when another part of the auricle sets the pace instead, but it also occurs spontaneously as the result of causes, the nature of which is not clear. It has been our good fortune to observe on a few occasions a gradual transition from *P* waves directed upward to *P* waves directed downward; if the apices of succeding *P* waves in such curves are joined, a curved line results.

This very curious phenomenon depends on an obscure mechanism, and unfortunately we have no satisfactory explanation for it. We know that where we have seen it best, it was associated with accidental intoxication by the drug digitalis. But we have seen indications of it in other connections which render its interpretation impossible with our present knowledge.

Some of the changes which are observed in the ventricular electrocardiogram are more easily explained. Considered as a whole, that is to say, in the light of the three usual leads of Einthoven (the first from the two arms, the second from the right arm and the left leg, and the third from the left arm and the left leg), one obtains a great many curves in which the waves in the first lead are inverted, and others in which inversion takes place in the third lead. Changes such as these result from a variety of causes. A heart which is not firmly anchored, but is easily shifted within the chest cavity by changes in the position of the body, may yield curves in which inverted waves appear. Comparable changes may result when the heart is pushed by air or fluid introduced in the chest, or when it is pulled to one or the other side by bands of adhesion stretched between the heart's surface and the chest wall. The explanation now offered for these phenomena by Einthoven and others is that the relation, determined by Waller, of the electrical axis of the organ to the body axis has become altered. Similar causes are probably at work in the electrical changes which are seen in increases in size of the heart, whether due to dilatation of its cavities, or to thickening of its walls; and the curves vary according to which side of the organ is involved. When the right side undergoes these changes, it is in the first lead that the ventricular waves become negative (Fig. 4); when it is the left side (Fig. 5), the negative waves appear in the third lead. Although alteration in the relation of the electrical axis and the body axis is the cause, that is to say, the mechanical, anatomical cause, commonly given for such deviations from the normal curve, it appears necessary to remember that in hypertrophy of the heart the balance of the sum of potential differences which produces the normal electrocardiogram must be disturbed, and that a rearrangement, that is to say, a functional rearrangement, of the parts of this sum must occur and might of course result in the changes we

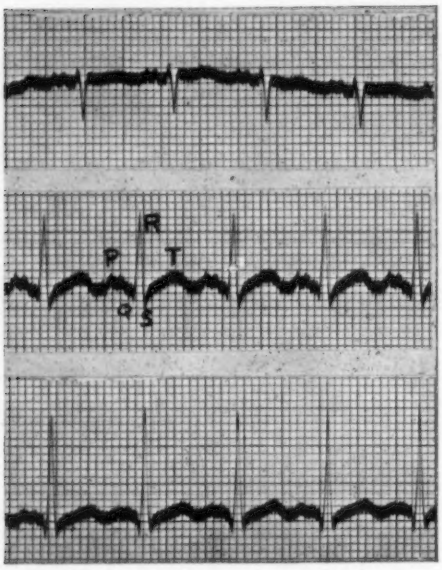


FIG. 4. Electrocardiogram showing the three usual leads of Einthoven. From a heart showing hypertrophy of its right ventricle.

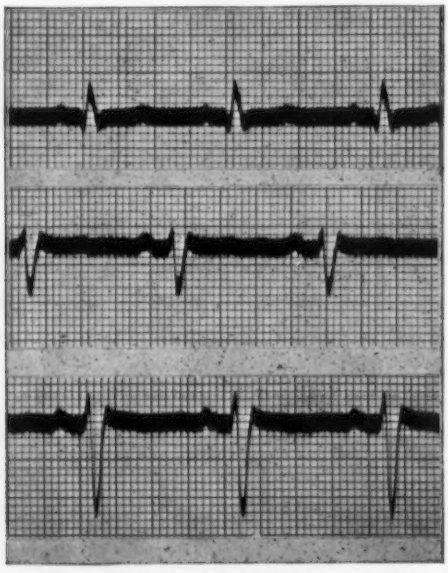


FIG. 5. Electrocardiogram showing the three usual leads of Einthoven. From a heart showing hypertrophy of its left vertricle.

are describing. More exaggerated changes still are observed when there is no alteration, either in the size or in the position of the heart; and these are due to the manner in which impulses are propagated to the ventricles from the contracting auricles. The pathway followed normally has already been described, but now the normal path cannot be taken, for it has been partly destroyed. It has been shown that when the conduction bundle to the right ventricle is

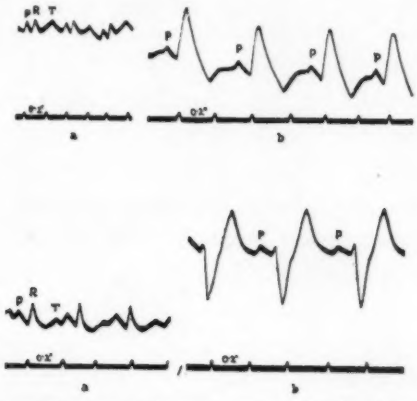


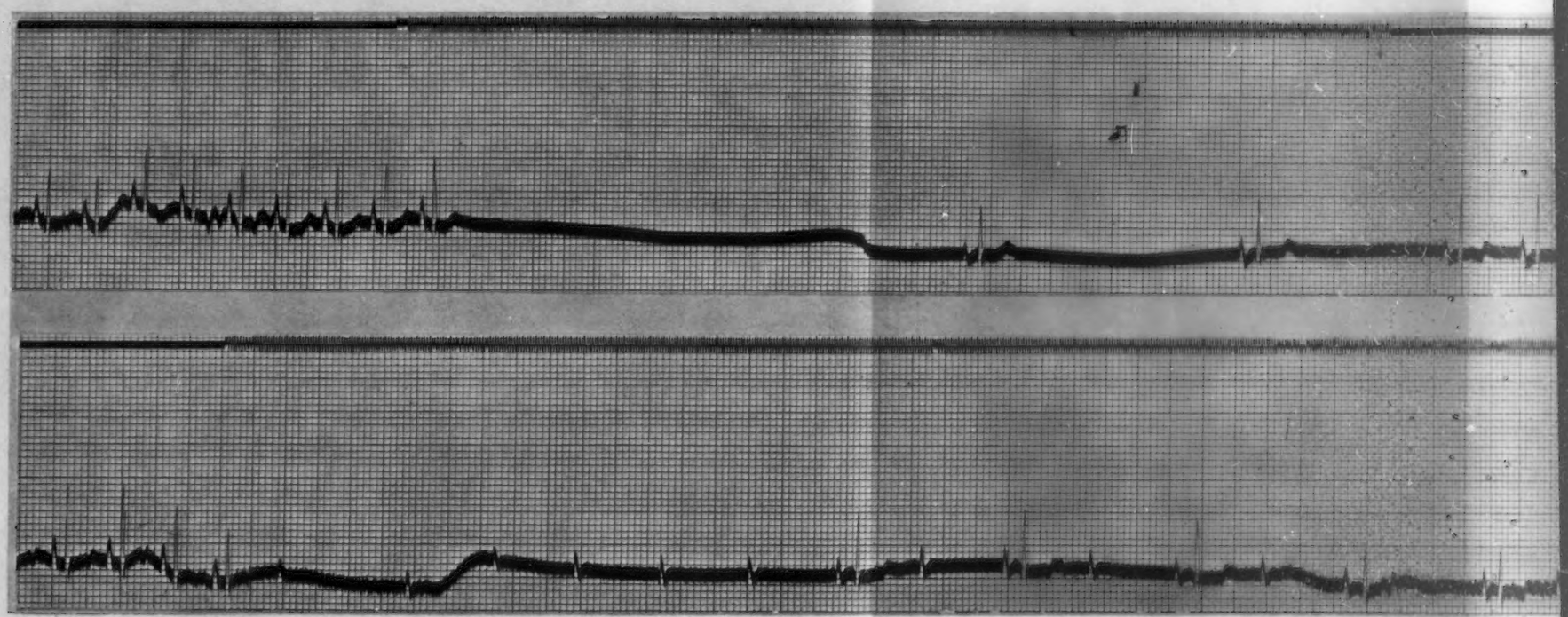
FIG. 6. Electrocardiogram from a dog. Leads from oesophagus and anus. Above: first portion is a control; the second is a curve taken after the left branch of the *A–V* bundle has been cut. Below: control and a curve taken after the right branch of the *A–V* bundle has been cut. After Rothberger and Eppinger.

severed, allowing impulses to reach only the left ventricle, the electrocardiogram immediately takes on another shape, the shape being an exaggeration of what occurs during enlargements of the left side of the heart. The first ventricular wave is sharp downward and the second an upward deflection. If, on the other hand, a similar injury is done to the conduction bundle to the right ventricle, there is a reversal of electrocardiographic curve. It consists of a sharp upward, followed by a downward deflection (Fig 6). Finally, if a curve of one or other form has been obtained by cutting one or other branch of the conducting bundle, one can, by severing the still uninjured branch, obtain an electrocardiogram which differs from both the preceding and resembles, though not exactly, the original curve. These changes depend, then, upon the way impulses pass through

the heart. An explanation of the nature of these abnormal curves is obtained by comparing them with others from hearts to which direct mechanical stimuli have been applied. A stimulus to the left side of the heart (ventricle) yields a curve like that seen when the right branch of the conducting bundle is cut, while one applied to the right side is like the curve seen when the left tract is cut. Under both conditions, contraction is initiated at the site of the stimulus, and the impulse spreads over the heart from this area. The form of the curve yielded depends on whether the right or left ventricle initiates the contraction.

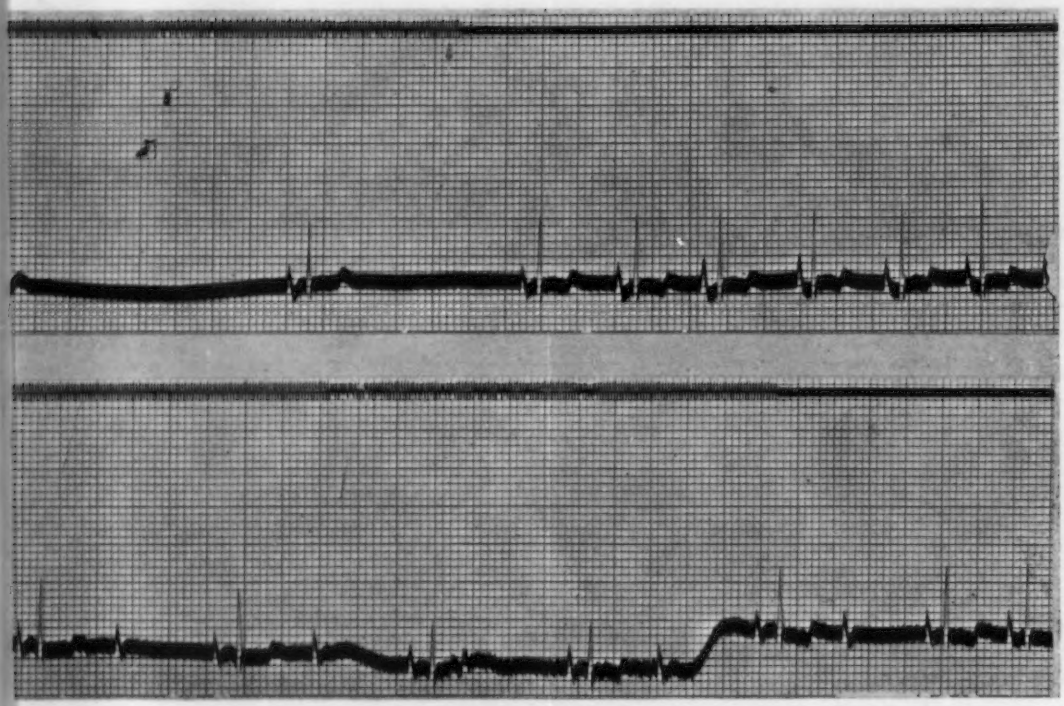
The conduction bundle, then, is an important factor in the orderly cardiac mechanism, but, on account of its exposed position in the heart, it is frequently injured. The pathway between the auricles and the ventricles is, therefore, interrupted and impulses from one to the other are consequently blocked. The ventricles, deprived of stimuli from above, contract independently and without reference to the rate or rhythm of the auricular beats. In the study of derangements of the cardiac mechanism of this nature, electrocardiography has rendered distinct service.

So far we have discussed the mechanism of the heart-beat only in so far as it relates to the intrinsic arrangements of the heart. But for the proper exercise of a number of its functions, the heart is subjected to the influence of the central nervous system. In the study of this influence the galvanometer has been not only useful, but essential. Branches from the central nervous system to the heart exercise functions of two sorts,—inhibitory and accelerator. It has been especially our work to show that the inhibitory or slowing function is not simple and is not possessed equally by both vagus nerves. To say that the right vagus nerve principally modifies the rate of the heart and the left vagus chiefly conduction between its chambers states a truth and also indicates the presence of a complicated mechanism which may be explained in the following way: The complexity depends on the fact that the heart was originally an unpaired organ with a symmetrically distributed nerve supply. This supply, so far as we are informed, was directed to the junction between the old sinus venosus and the auricles. In the development of the heart, a division of the junctional tissue took place; one portion, the right,



Electrocardiogram from a dog. The upper curve taken during stimulation (signal above) of the right vagus nerve. The lower curve taken during stimulation of th
ordinates are 0.1 millivolt; of the abscissæ, 0.04 seconds.

PLATE II



vagus nerve. The lower curve taken during stimulation of the left vagus nerve. Divisions of the

remained at its original level, but became incorporated in the wall of the right auricle as the sinus node; the other portion, the left, became dislocated and moved downward the distance of a whole chamber, becoming, in the adult mammalian heart, the auriculo-ventricular node which lies between the auricles and the ventricles. The importance of these changes in position, from the point of view of innervation, lies in the fact that the cardiac branches of the two vagus nerves, which were distributed symmetrically in the primitive heart, have followed the changed positions of the structures they innervated originally and have become incorporated with them in their new situations. Consequently, the nodes and their nerves have assumed different functions. We were led to believe that this change must have taken place, first by clinical observation, and we have tested this hypothesis by experiment. As the result of experiments on many dogs, we were able to decide that when the right vagus nerve was electrically stimulated, the heart stopped beating. But when the left vagus nerve was stimulated, the auricles did not stop beating, but continued, though often at a slower rate. The striking thing now observed was that these auricular beats failed, either entirely or only occasionally, of being followed by a ventricular response (Plate II).

These were the facts. In the light of current teaching, the motions of the heart are initiated and rate is maintained by the sinus node. Impulses so initiated are conducted from the auricles to the ventricles over the narrow conduction path, with which we have become familiar. It follows that, when we find the heart stop as the result of a stimulus, we must assume the stimulus to have been distributed to that portion of the heart where the pace-making function resides, that is to say, at the sinus node. In the same way, when a disturbance in coördination between auricles and ventricles occurs, we have sufficient evidence to indicate that this occurs as the result of an effect produced at the junctional connecting tissues. We must, therefore, conclude that if stimulation of the left nerve brings about this disturbance, it must necessarily be distributed to this portion of the heart, that is to say, to the conducting system. Although we think that these sites, the sino-auricular and the auriculo-ventricular nodes, are the main terminals of these nerves, it is clear that other functions

are influenced at the same time when they are stimulated. We have indeed shown that this is the case. But our main concern has been to obtain information about essential differences; the likenesses and the additional influences exerted will be apparent. In this problem again, the galvanometer has been invaluable. On the basis of ordinary mechanical records, of which we have made many in the course of this work, we could not have drawn the conclusions just detailed.

Stimulation of the augmentor nerves shows that these have distributions similar to those of the inhibitors. Rothberger and Winterberg have contributed these facts. They have shown that stimulation of one or other augmentor nerve produces effects which can be referred to a modification of the function of the special cardiac tissues we have discussed in connection with the inhibitory nerves; and they have also shown that other differences of an electrocardiographic nature take place.

We have traced, in recording the newer aspects and methods of the study of the heart-beat, the influence exerted by the introduction of electrical methods. Advances by this method were due in large measure to the construction of an adequate galvanometer. But the advances in our understanding of the heart have depended on detailed anatomical and physiological investigation of the newly discovered structures in the heart itself. How small a portion it is that has been studied in relation to the whole heart, and how relatively few functions of the efficient organ have been included in the recent investigations, has been indicated. Much remains to be done by the means at our command, but much also by others still to be devised.

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THE USE OF A PHOTOGRAPHIC DOUBLET IN CATALOGUING THE POSITIONS OF STARS.

By FRANK SCHLESINGER.

(*Read April 24, 1914.*)

In order to utilize for cataloguing and for similar purposes the positions of stars derived from photographs, it is necessary to know the scale of the plates, their orientation and their positions in the sky. In the early days of astronomical photography attempts were made to determine the scale and the orientation by such methods as measuring the absolute focal length of the photographing telescope, and by impressing an orienting star trail upon the plate. These attempts have not been successful and it has now become the universal practice to employ comparison stars for these purposes: that is, stars that appear on the plate with positions known in advance, usually through observations with the meridian circle. Here again experience has shown that in the determination of star places by photography, this matter of comparison stars is usually the weakest link in the chain. For example, in the case of the Astrographic Catalogue much of the relatively high precision with which the plates can be measured is lost on account of the lack of suitable comparison stars.

An experiment that aims to overcome this difficulty is in progress at the Allegheny Observatory. Instead of employing a simple objective to photograph the stars, a doublet lens is used. This has the advantage of much greater extent of field of good definition, at least six times as great as in the case of the objectives used for the Astrographic Catalogue. Consequently in surveying any large area of the sky, plates taken with the latter kind of telescope will require (other things being equal) six times as many comparison stars as the doublet.

The use of the doublet for these purposes was advocated twenty-five years ago by Pickering, but astronomers have feared that the use of such an objective might introduce serious errors of various kinds. The experiments already completed at Allegheny prove that,

with proper precautions, these fears are groundless and that this form of instrument is admirably adapted for cataloguing purposes. The detailed results of these experiments will soon appear as No. 9, Volume 3 of the " Publications of the Allegheny Observatory." On this occasion it will suffice to give merely the results.

A number of regions covering about twenty-five square degrees each, were photographed in duplicate, first with the center of the plate at the center of the region and then with the edge of the plate in that position. A comparison of the two sets of positions gives

$$0''.18$$

as the probable error of the measurement of one image. This quantity includes not only the accidental error but the following as well: (1) outstanding errors in the measuring engine; (2) the optical distortion of the objective, due to a possible failure of the objective to give a truly linear reproduction of the object photographed; (3) magnitude distortion due to spherical aberration and causing bright stars to appear systematically nearer or systematically farther from the center than faint stars. The result shows that all of these errors are very small, and additional experiments indicate independently that (2) and (3) are negligible or very nearly so.

Doublets have been extensively used in astronomy for pictorial purposes. The doublet that we have used for the above experiments differs in one important respect from these: the ratio of the aperture to its focal length is twenty-one instead of five or six This circumstance is favorable to its performance over a wide field and is doubtless responsible for the smallness of the optical and the magnitude distortions.

Our objective covers well a field of twenty-five square degrees, as compared with four square degrees in the case of the plates for the Astrographic Catalogue. For the latter, as already stated, six times as many comparison stars would be necessary in a large piece of work in order to determine the plate constants equally well. The scale of the astrographic plates is a little more than double that of ours, and the purely accidental error of measurement is therefore somewhat smaller, but not as much smaller as this difference in scale would imply. In practice, so much of the accuracy of photographic positions depends upon the comparison stars, that if a large area

were to be surveyed with both instruments and the same comparison stars were used in the two cases, our doublet would give the more accurate results. If there were at hand in some special case comparison stars that are much superior in number and in accuracy to those that are generally available, then the astrographic plates would give the better positions. Needless to say, a doublet like ours with twice its aperture and twice its focal length would give still better results, but this would necessitate plates about 35 centimeters square and a measuring engine large enough to accommodate them.

Our objective would also be well adapted for compiling zone catalogues of faint stars similar to those of the *Astronomische Gesellschaft*. The latter now extend from 80° north declination to 18° south, a work that has required the cooperation of sixteen observatories during a considerable number of years. Observations are in progress that will extend this catalogue farther south, but no provision has yet been made for the southernmost zones. When these come to be actively considered, the claims for the doublet should be carefully weighed. The original plans for the Gesellschaft Catalogue included the repetition of the observations after the lapse of about half a century. This time is now approaching in the case of some of the northern zones, though others (notably the one between 70° and 75°) are of more recent origin. The repetition of all the northern zones, if carried out photographically by means of a doublet, would be a task well within the powers of a single observatory. Moreover there would be a very considerable gain in accuracy. From the prefaces to the various zones of the Gesellschaft Catalogue we learn that the probable error of one observation in either right ascension or declination is on the average well over 0″.50. This was derived from observations made with the same telescope on different nights, and does not include certain errors that would be brought out by comparing observations made at different observatories. Our plates yield for the probable error of one observation, 0″.18. This was obtained by comparing overlapping plates, but it does not include errors due to inaccuracies in the positions of comparison stars.

Although the tests we have made with our doublet may be ac-

cepted as indicating the size of accidental errors and of certain kinds of systematic error, it cannot be claimed that they tell the whole story. To make certain that star places thus derived are free from serious systematic error of any kind, would require much more observational material and would involve certain extensive intercomparisons as well as comparisons with star positions derived by wholly different methods. Such a work is contemplated at Allegheny and is in fact well under way. The zone extending from declination 2° north to 2° 10′ south is being photographed in duplicate, the centers of one set of plates being upon the eastern or western edges of the other. Each plate will embrace 24 minutes in right ascension or 6°; the program at the telescope therefore calls for 120 plates. To determine the plate constants 602 comparison stars will be employed, an average of ten on a plate. The mean of the two positions for each catalogue star will therefore depend upon about fifteen comparison stars. Thanks to the courtesy and cooperation of Director Campbell and Professor Tucker of the Lick Observatory, the positions of these 602 stars are being determined with the meridian circle of that institution.

Within the limits of declination selected we are measuring the positions of all the stars that appear in the three Gesellschaft zones that overlap. The observing list for the latter was made up of the stars that are designated with (visual) magnitudes 9.0 or brighter in the Bonn Durchmusterung, and as many fainter stars as circumstances permitted. In our work we shall necessarily omit a few stars that are photographically faint by reason of their color, a few stars that are so bright as to present images too large for accurate bisection, and a few doubles that are too close for separation on plates of this small scale. Making allowance for these omissions the completed catalogue will contain about 7,100 stars. It is hoped that this work may not only prove a valuable contribution to our knowledge of the positions and motions of faint stars, but that it may enable astronomers to decide definitely as to the advantages and disadvantages of this form of instrument for the wider applications of the same kind that the future will demand.

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SOME OBSERVATIONS ON THE PSYCHOLOGY OF JURORS AND JURIES.

By PATTERSON DuBOIS.

(*Read April 23, 1914.*)

Interpretation is the crux of science and the chief point of failure in scientific men. When concerned with human behavior it reaches the most subtle nicety—a kind of nicety, indeed, which projects far beyond the helping hand of instruments of precision or mathematical formulæ. Human interpretation is the pivot of human intercourse.

Under the prescriptions of our common jurisprudence the verdict of a court jury is mainly a resultant of many subconscious forces and unseen influences. Standing obviously to reason, this needs no demonstration. Comparatively little of "the evidence" admitted by the court during the trial and little of the pleading operates directly upon the mind of the jury—even though, paradoxically, without these there could be no trial.

It is no part of this paper to attempt the impossible task of unravelling the real or probable complex of past experiences, prejudices, emotions, misunderstandings or logical balances of judgment affecting the minds of the jurors.

Chiefly, it is limited to the proposition that, so far as the juror is concerned, court discipline tends to diminish rather than to increase, and to hamper rather than to facilitate his efficiency as an agency of justice. In other words, the conscientious juryman of good average equipment goes handicapped to his task largely because he is in subjection to an iron rule which takes no note of his individuality of fitness or unfitness, distress or ease, or of other personal and court conditions which themselves condition his judiciality. The problem is largely one of attitudes—a comparatively neglected field in pure or applied psychology.

The court takes cognizance of certain classes of influences likely

to affect the jury mind, giving way to some and aiming to suppress others that tend to prejudice and disturb the equanimity which makes for justice. But the main thesis here is that there are other and subtler influences which are often a still greater menace to justice and that there is little thought of reducing them to a minimum.

Manifestly, this should be the first consideration—just as the reduction of friction and of waste effort are of prime importance to the mechanist. Similarly, a business requiring so much personal sacrifice, such delicate mental balance coupled with resolute and courageous control, such openness to conviction without the weakness of hasty consent,—such a business should be carried on under the most favorable conditions instead of under conditions tending to defeat the desired ends.

In his delicate and often vital responsibilities everything should, as far as possible, conspire toward the best use of the juror's morally controlled intellectual judgment, whereas this judgment is subjected to an automatic conspiracy of obstacles, infirmities, and irritants. He is expected to be, and, in my experience, generally wants to be, free of all forms of bias for which the challenging attorneys take little thought. The juror, indeed, is in no small degree a marionette of which his subconscious self holds the controlling wires while the whole inherited system of trial by so-called peers is the grinning instigator. And the marionette is seldom wholly self-respecting.

All the way down from the earliest English times when witnesses were jurors and jurors were witnesses, gathering their own testimony and acting on their own knowledge, there have been many modifications of the jury system and diverse views as to its value.

The juror's office is, in some respects, the most ironical and paradoxical in the whole range of human service. Its requirements are those of a learned profession with demands of extraordinary versatility. The profession is to be followed by the individual for a very brief period with uncompensating "compensation" (often at an actual loss to the juror.) Yet the juror is so hedged about, officially suppressed and oppressed that the opportunity for initiative, the value of his knowledge, and the freedom of his judgment are reduced to the minimum instead of being employed at their maximum. He is, in effect, both puppet and prisoner.

I am especially considering the juror of the more intelligent and selected class. As a rule, he admits that jury service, in spite of its drawbacks, is educationally profitable and often interesting. He means that his duty shall be conscientiously performed. He is sensitive about sitting in judgment upon his fellows—for he knows his own liability to appear at any time as plaintiff or as defendant.

Yet withal, he is asked to swim with a stone on his neck, to be agile in a straight-jacket, to be patiently complaisant in duress—a mental Blondin in hobbles.

His bristles begin to rise, when, in his own home, the summons is served with a threat. The "law" first greets him with flashing eye and teeth uncovered. The more conscientious and honorable the man the more offensive appears the attitude of his legalized captor.

In the United States courts this summons may compel the victim to attend court fifty or a hundred miles away. He may live on a branch road where train service is such that he could not go and come every day. Even if he could, he would be out of pocket since the fee is insufficient to pay the passage. This condition may require the juror to be absent for a week at a time from his business, or from a sick wife, virtually without compensation and really without assurance of release in emergency. How can a man's judgment be at its best under such circumstances?

In any case, whether he lives far or near, the average man on receiving notification that he is wanted is, for the moment at least, disconcerted or timorous. Before him rises the giant of business demands, home dependents, personal physical disabilities or pathological possibilties, a sense of being penalized, of loss of freedom without cause, a danger of "contempt" through sheer ignorance and inexperience, a vision of all-night incarceration with uncongenial strangers, perhaps sickness in his family unwaited upon and beyond communication with him. I doubt whether any jury panel is ever drawn entirely free from some or all of these mental perturbations in some or all of the individuals. They are, however, especially true of the first-time juror. To add to his discomfiture his neighbors either pity his plight or make a jest of it or advise his making an effort to get excused. I have talked with two classes of drawn

men. One never expects to serve if release is possible, another chafes under his weary durance yet says that he ought and means to serve if possible.

Once in the traces and more or less accustomed to the routine the first pressure of the screws is less felt. But, none the less, the juror has a mind and a body. He does not forget that he was born a freeman nor that he is absent from farm or store or office. The delays, the wearisome reiterations and professional fencing, the interruptions and general unbusinesslike pace of procedure are at his cost. They irritate and disgust.

When the juror is drawn from the "panel" to serve on a particular case he is ordered into his seat with pretty much the same show of authority and sense of subjection as he sees when the accused defendant is policed into his presence. In an interesting reminiscence of long and varied jury experience published in a magazine article some years ago Mr. Joseph Hornor Coates speaks deprecatingly of these indignities:

> "The juryman from his first entrance in response to the court's peremptory summons finds little in his treatment to impress him with an idea of special dignity in his position, even if he has no overt cause of compliant. He is herded with his fellows, ordered about by the tipstaves or bailiffs of court, addressed in peremptory tones . . . . He sits in the court room with an ever-present sense if he be sensitive, that he must be careful not to get into trouble; the feeling of liberty is gone, he is enveloped in an atmosphere of restraint. Really, he is placed more on an equality with the prisoner at the bar than with the judge on the bench, yet he is as essentially a part of the court as that august potentate and may have at any time a greater responsibility imposed upon him. In some court rooms when disengaged from the actual trial of any case and awaiting summons to the jury-box, the juror is often forced to sit among criminals, witnesses, loafers and ill-smelling persons attracted to the court by business or curiosity."

While the average juror resents these low estimates of his office he does not, perhaps, fully realize how his own judicial faculty is lowered by his lowered estimate of the court as an institution. He becomes critical and dislikes being party to the system.

The juror's disposition to criticize the system (rather than court officers themselves, for the officers naturally fall into the formal bondage of a system) increases through the meanderings of the trial, —the surplussage, the objecting attorney, the lust for filing ex-

ceptions, the hectoring and heckling of witnesses, the muteness of the jury itself, and finally the mistrust of its honor in locking it up to reach an agreement—not always real but formal—all these incite the sensitive and the honorable mind especially to secret rebellion against the system. This state of mind cannot but endanger verdicts.

There are other influences of entirely conscious and purposive kind which affect verdicts too. Thus, in the jury's deliberations under lock and key a juryman will openly confess that he will not vote to acquit a negro prisoner because he *is* a negro; or he will stand by a chauffer in a suit for damages because he himself drives an automobile; or he is moved by fear of the judge to decide against what he has ordered or what he believes to be the judge's opinion; or he is politically afraid; or he dislikes to be the target of ten or eleven others; or he wants to go home, to take the next and last train, perhaps, to avoid another night at a hotel; or he cannot understand the case—its technical terms, its arithmetic, its alternatives, its fine-spun distinctions, some of which are purely technical and have no direct bearing on justice.

But while some of these influences arise directly out of the system and are corrigible they are mostly more obvious and overt than the subconscious causes of mental sway, arising immediately out of juridical prescription and attitude. Some of these have already been noted. Timidity, restraint, sense of personal loss, personal discomfort, offensive environment and treatment, worry over private troubles caused in part by absence from home or business —all of these affect the balances of a sensitive mind and are in a large measure corrigible. The implication that as a juror one cannot be trusted to come and go while the judge and the witnesses and attorneys are free hurts a sensitive mind and excites a rebellious spirit.

In fact, the wearisome reiterations in the examination and cross-examination of witnesses, the time consumed in legal fencing, the objecting and excepting, sometimes give the juror a sense of being unfairly exploited for the gain of attorneys rather than for the settlement of real difficulties. Juries are jealous of their time and personal freedom; so surplus verbiage and legal loquacity irritate to

the extent of damaging a cause—for it is a fact that a verdict or a disagreement is not seldom a slap at a lawyer's course. Many a case is undone by overdoing. This is a hard lesson for lawyers to learn. In his reminiscences of a long life at the bar Theron G. Strong (*The Outlook*) notes this danger of time-wasting excess and overdoing. He says:

> "The man who can say the most good sense and sound law in the shortest time has a decided advantage. Juries are not much influenced by outbursts of eloquence, and appellate tribunals will not tolerate them. A tired and yawning jury will not be likely to take the most favorable view of an advocate's case, and when the attention of an appellate tribunal is lost and the judges begin to converse in whispers or bury themselves in the record, the oral argument is little more than a waste of time. When you have lost attention, you have probably lost your case. Juries and judges have become so accustomed to business-like methods that they appreciate a simple and clear presentation of the essential facts, each argument in its support clearly stated in a few well-conceived sentences, with no haltings and no revertings to things inadvertently omitted, no fumbling of documents, and no reading from authorities . . . .
>
> "One of the most important arts of the court lawyer is to know when to keep still, and be able to exercise the self-command to do so. Many a case has been won by paying due regard to the attitude of the judge when he essays to combat the views of opposing counsel. The lawyer is indeed wanting in tact and discretion who then assumes any other rôle than that of a spectator of the proceedings. By all means let the judge do your arguing for you if he is so inclined, and if in this way he indicates that he is favorably disposed it is folly to attempt to reinforce his views; even though they could probably be reinforced to advantage, they do not need reinforcement so long as he adheres to them. The moment the court appears favorably inclined to your side of the case is the time to preserve discreet silence. This is equally true with juries, and if in the course of the trial there is the slightest leaning in your favor, then is the time to do as little as possible by objections or long cross-examinations, which can only have a tendency to lead the court and jury to think that you consider it necessary to strengthen your case when it needs no strengthening, the only effect being to counteract the favorable impression that has been made. Many a case has been spoiled by an inability to recognize the appropriate time to say nothing."

I quote this at some length because few lawyers are so discerning of the juror's point of view. I confess to the feeling of an oncoming bias against the lawyer who is working too hard. Too much repetition of evidence, too many witnesses, too great detail in the pleading, too much swelling molehills to mountains, too noisy oratory—all these excesses tell on the juror's temper. He does not

care to be treated as though he had not the brains to see the bearings of the testimony as it came from the mouths of the witnesses.

The court is quite right in sustaining objections to much extraneous matter that clogs the proceedings and overloads the jurors. But it is questionable whether the loss of time by a large majority of these objections does not tend to irritate jurors who are rightly jealous of their time and critical of a system which compels its loss without corresponding gain to them. Much of the matter ruled out or admitted after a battle between attorneys goes for what it is worth to the jury and not a little of it is without effect on the juror's mind either way, except as a general irritant. Whatever is evidence to his mind *is* evidence and willy-nilly his mind is affected by it. To a juror with imagination and the gift of interpretation evidence is often felt as atmosphere and is much more than the dry bones of admitted testimony.

For instance, in a trial in which the verdict turned chiefly on the motive or purpose of a paper engaging to purchase and pay for a large block of stock on a certain date, the lack of frankness and the constant evasiveness in one of the witnesses so strongly discredited him in the minds of at least half the jury that they virtually agreed that they would not employ such a man in their own businesses. Indeed the other half of the jury did not defend him. As this witness substantially agreed with and was on terms of more or less intimacy with the four others who told the same story the whole five were greatly weakened as witnesses. This was evidence unavoidable in coloring the minds of jurors, although not "evidence" on the record.

Again, in the case of a woman suing for damages as the alleged result of a fall by a defective brick pavement, more than one juror believed that the defense lost an opportunity in not bringing out in cross-examination the height and slope of the heels of her shoes—as these might have been more responsible for her tumble than the pavement itself. But as these jurors did not know anything about it—nothing having been said in court about it—they of course did not openly consider it. But the point is, that even such a case no one knows how far such a passing thought gives cast to the mind

consciously balancing itself to a nicety of honesty and absolute justice. It becomes evidence.

It is questionable whether the cause of justice would not be better served by a much more liberal attitude toward the admission of evidence than our jurisprudence usually allows.

This particular case is also a good illustration of jury-irritation through excess of detail and repetition. Large photographs were displayed and re-displayed and pencil-marked and passed and re-passed among the jury to show the pavement and the curbstone. All witnesses but one corroborated what the photographs were supposed to picture and no juror or any one else probably had any doubt about the pavement or about the slipping of the woman's foot. But it would have been just as effective to show the photographs once and for all, and obtain a simple statement of fewer witnesses without such over-elaboration.

There was a juror, for instance, who was suffering from a racking cough, who lived on a small branch railroad at perhaps fifty miles distance. He had left many business interests. It was now the last day of the term—Saturday. If he lost a certain afternoon train he would lose a whole day in getting home. He knew that a team had been sent for him a dozen miles or so through a snow-drifted country and if he could not catch that train his non-arrival would not be understood. Naturally he looked repeatedly at his watch when every senseless repetition delayed the trial. The judge did his part to push the trial through but there is a limit to the judge's action at least as a matter of expediency. The juror with dismay saw his train-time go by with suppressed irritation. One or two others from other counties were similarly affected. The insistence here is that the trial could have been gone through within half or two thirds of the time it really took. The jury knew no more for the redundancy and were less fit for the balance of their powers than they would be under more favorable conditions. Few men can be at their best as dispensers of justice when they see their valuable time frittered away—for what? To settle a contention in which they have no live interest and for which they are held in duress as though they were not to be trusted out of sight.

Mr. Coates's experience agrees with mine—that the pleadings

of the lawyers have very little direct effect on a substantial jury. In simple cases not involving too much consideration of human nature or of the motives perhaps most jurors catch the drift of the evidence and make up their minds before the trial is through—even though they suppose themselves open to conviction either way.

Lawyers damage their cases—sometimes lose them, by what the jury regards as an unfair trick. Some jurors are affected by the fact that a famous lawyer is on the case—his reputation alone carrying weight.

A very slight thing may bias the juror. One lawyer lost favor with some jurors because of his continual smile and offhand pleasantry in his pleading. Another quite properly asked one of the jurors, whom he called by name, to define a technical business term —the lawyer being or assuming to be not quite sure of it himself. This fact was noted in the juryroom and charged up against that juryman's opinion—one of his confreres asserting that apparent friendship with the plaintiff's attorney affected his judgment. This charge was entirely good natured but was given to offset a charge that the juror favoring the defence was partisan because of his local affiliations—the defendant hailing from the juror's locality. The jury disagreed in this case which was one of great difficulty because of local interests, human motives, and the large losses and gains involved. And yet no one can question the honesty of every juror there —while also no one can help questioning the amount of coloration in the minds of not a few of the jury arising from these causes apart from the formal evidence. It was noteworthy that all the jurors who were of a certain foreign descent and understood the same foreign tongue although not previously acquainted with each other, voted together. An unconscious esprit-de-corps arose which banded them solidly against the other half of the jury. I do not think that either side noticed this fact but I am quite sure that the subtle influence of nativity and speech worked on them without their knowing it. Of course no court could foresee or suspect or avert this. I instance it only as an illustration of unconscious congregate influences on honest and conscientious but untrained minds.

I have been strongly impressed with the comparatively helpless situation of the defense in many cases of suit for damages especially

when the plaintiff is poor and the defendant in comparative affluence and still more especially when a corporation. Quite apart from the traditional attitude toward the so-called soulless corporation a class of circumstances may put the defendant at a great and unfair disadvantage.

The case of the plaintiff who slipped on a curbstone and claimed that a sunken brick pavement compelled her to so step on the curb as to slip, is in point here. The very fact that the photographs are handed about and continually referred to, that witnesses have seen that condition there for years, that the woman's leg was so sprained by her fall as to prevent her making her living as heretofore gradually works upon the minds of the jurors because no one can say that none of these things are so.

I remember seeing nothing very bad in the photographs at first but the incessant references to the lines of the picture (I now see) exaggerated in my own mind the dangers from such conditions at a crossing. Some of the jury were particular to say that they believed in discouraging the legal traffic in damage suits but they also believed that sidewalks should not be allowed to become a menace to the walking public. Small compensation was therefore agreed upon by the clumsy process of averaging the vote.

A few weeks later I went, out of curiosity, to see the place itself. I am quite confident that as a casual pedestrian the slight sinking of the bricks and the slope of the curbstone would never have attracted my notice. The photographs were in a sense true and in a sense untrue. The proportions of the street were distorted and the viewpoints were selected for a purpose. Much was made, at the trial, of the rounded curb which had been worn that way by the grating of heavy wheels against it on the corner. The rounding never looked bad to me on the photograph but as I think back to an objective view of my own mind during the trial, excusing the photograph's supposed untruth as being too small or perhaps not properly posed to show the real danger, I see how gradually I began to think against my real judgment and see untruly. And now in its very presence the stone itself looked no worse to me than did the photograph at first; and I am bound to wonder why if this pavement and curb were entirely to blame there are not such accidents going on all over the

city every day, for I notice that that pavement was relatively not dangerous or bad.

I am not quarrelling with the verdict in which I had a part. It was not very serious either way except as a matter of abstract justice. I am merely trying to show how much more those pictures, in conjunction with the strained reiterations of witnesses, counted for than they were really worth. The mind believed what was true in them because they were photographs and excused and apologized for what seemed untrue (but was really very true)—also because they were photographs.

In an article on "Photography and Crime" Mr. C. H. Claudy says:

> "Any capable photographer knows how to magnify or minimize certain parts of the perspective of any view. Thus, a long-focus, narrow-angle lens will give a totally different result from a wide-angle, short-focus lens. In a suit for damages because of obstructions left upon the street, for instance, a lawyer will have a photographer use the latter lens and stand close to the alleged obstructions. A pile of earth, particularly if photographed low, will appear very large in proportion to the vanishing perspective of the street. A natural-angle photograph, made with a ten-inch lens on a five-by-seven plate, will give a totally different idea of the size of the obstruction.
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> "Cracks in buildings, as evidence of the damage done by subway construction or sewer-laying, can not be brought before a jury; but photographs of them can be so used as evidence. A clever photographer, by manipulation of his illumination, so that one side of the crack throws a heavy shadow can make such fissures appear far larger than they really are. Pictures of hills, to show the locality of a runaway, can be made steep or flat according to how the camera is handled. It is not, therefore, necessary to resort to actual changing of the negative and print to make the camera deceptive, and more and more are our courts coming to understand this fact."

I have been startled several times with the seeming unevenness and bad brick-laying in a brick extension wall of my own house. In early summer the low afternoon sun throws long shadows lengthwise from irregularities in the brick quite unobservable at other times. A photograph of the wall at such a time might be shown as strong evidence that the wall had suffered some kind of disruption. Yet the fact is that it is finely and evenly laid.

No allegations of attempt to falsify in the case under consideration are here intended. It is true that in the pictures the little streets looked broad and fine but the defects of the pavement and the curb

were not exaggerated. The point is, that when photographs are shown in support of the verbal testimony even the discriminating mind is apt to be over-persuaded by the mere fact of pictures. Any seeming lack therefore on the part of the picture is subconsciously excused on the ground that a depression of two inches in a pavement is necessarily diminished to almost nothing in a photograph. Then the mind rebounds to an exaggeration of the truth notwithstanding the claim that the depression does not exceed two inches.

Manifestly, the defense has little show in such a case. He cannot prove that the plaintiff did not fall from this cause. His one witness denied the dangerous character of conditions and to us jurors this denial seemed fatuous and partisan. But when I saw the place itself I thought this witness more right than wrong—more true indeed to the moral fact than all the others.

I am quite sure that had the jury been taken in a body to the actual scene of this accident the outcome would have been different. I could scarcely believe my own eyes. I tried to slip on the curb as the plaintiff slipped because of the slightly sunken but not rough pavement, but I failed. True, I had rubber heels but true, also, the woman may have had suicidal "French heels." Of this we jurors had no knowledge but some of us thought of it. All the verbal testimony of many witnesses corroborated what the photograph was supposed to show. It did show lines and shadows but not danger. The witness said danger and the jury believed that the photographs showed it too. Doubtless, also, the degree of damage which the woman suffered became, sub-consciously in the jury mind, the measure of that danger. I see now, that the pictures did not prove danger —not relatively, at least, as I find pavements and curbs wherever I go as bad and worse but they do not seem to me dangerous. If the defendant was guilty of negligence, comparatively few property holders are not guilty.

This case has been reviewed at some length because of its illustrative values in pointing out how the mind shifts itself under the subtilties of "evidence" which is in reality no evidence, but which cannot be denied or assailed as untrue and cannot be easily ruled out.

It seems to me that with a knowledge of the psychology of the gradual winning of the juryman's mind in spite of his own better

thinking, appeal for retrial should have serious consideration by the judges. In such a case the defense in not weak because the lawyer is weak, not because the defense has not been properly worked up but because all the activity, all the pictoriality, all the interest, lies with the plaintiff. One woman slips; a hundred thousand do not slip; but they never come on the witness stand. Moreover, the mind assumes that unless the case were good no photographs would have been taken, as the risk of recoil would have been too great.

In cases where medical experts are called to testify lawyers are too much inclined to display their crammed knowledge of the anatomy and physiology of injured limbs. Direct and cross-examination in these cases are sometimes carried to absurd length and minuteness. It makes little difference to a jury how a nerve moves the muscle and how the life process itself produces movement. I am quite aware that some of these foolish professional displays are made with the hope of discrediting the opposite expert witness but as a rule they are wearisome and even ludicrous—and neither of these conditions helps the cause.

Whatever makes for straightforward simplicity counts for the jury's favor. It is then that the real evidence weighs at its true value. Of course there are jurymen who admire adroitness, shrewdness, cleverness, even craftiness in an attorney and are much influenced by that admiration rather than by the real merits of the case. But these very attainments or methods work the other way with many jurors; and the insolent brow-beating of witnesses or manifest effort to confuse and " rattle " a simple-minded and honest witness is pretty sure to awaken indignation in the jury and recoil on the parties indulging in that kind of practice. This indignation is no part of the evidence which jurors are sworn to decide the case upon but it goes for evidence as weight in the mental balance.

And on this point, when the court orders a specific verdict without consulting the mind of the jury, is it not virtually ordering possible perjury or, in effect, subornation? Seeing their liability to this kind of termination to their labors jurors sometimes grow lax or indignant, according to their temper. They become puppets either way and official " evidence " becomes of less moral moment in their service.

In point here, is a paragraph from *Law Notes* (March, 1910):

"Judge Caldwell who had served nearly 35 years on the bench of the Federal District and Circuit Courts said that trial by jury was guaranteed in the Constitution 'because the people knew the judges were poor judges of the facts' and that 'every day's experience confirms the wisdom of their action. Equally strong testimony has been given by some of the greatest judges this country has ever known,' . . . . How many self-respecting men will condescend to serve willingly on juries if they know the judge is likely to hold them up to public scorn because he disagrees with their unanimous opinion delivered under oath?"

Now let us add to this question the weight of the fact that the greater the intelligence and moral force of the juror the greater his antipathy to such an unjust service. And in his resentment lies the psychological menace to his natural qualifications. And surely the lower grade of man is not to be preferred because of his being the more subservient.

Not having sufficiently investigated the claim of the incompetency of judges to judge of "facts" I make no comment on it—except that apriori grounds seem to me to favor it. But if it be true then many a case suffers (as indeed every juror knows) from the exclusion as well as the inclusion of testimony. If the juror is a better judge of "facts" than the judge is then the juror should have power to call for such facts as appear to him to bear on the case. I for one have heard witnesses choked off by the incessant objecting of attorneys until the testimony was squeezed dry of all that essence which gives to a story its true value. True, a witness may overcolor and may run into imaginings and expeditionary sallies of sentiment and statement and this freedom should be limited. Nevertheless, the dry skeleton of what the court calls "facts" is often as untrue by default of important facts as the overweighted story tends toward giving an untrue impression of the case.

The juror should not be submitted to the strain of shutting out what he heard ruled out by the judge as not being relevant or admissible as evidence. I remember hearing a jury debate the import of an offhand written promise to pay a large sum. A youthful juror reminded us that the judge had said that we could consider the oral testimony in such a case. At least half the jury disregarded the paper because the judge permitted the oral testimony to count. The

other half saw in the paper the most vital evidence not, however, because the judge admitted it but because it appealed to their sense of business honor. Another judge might have ruled it out entirely as it lacked some factors of a formal document. What is evidence to some is not evidence to all, and no line can be drawn for testimony. Should not jurors have some right to demand such information as seems to them to be evidence? The "rules of evidence" are sometimes as wooden as they are usually useful. The mind of the average jury is in danger of being befogged by the ins and outs of matter offered as testimony.

It is true that a jury has more power than it usually takes in examining witnesses or inquiring of the court. But that is in large degree because the juror feels his automatism and his incarceration. Many a juror would like to ask questions but he has not been invited and he is afraid of attitude. He works under threat and all penologists are agreed that timidity and fear under threat do not make for strong intitiative or moral control.

In no situation in life is what Bergson calls the "professional comic" more evident than in the court room. It is always conspicuous in convocations of the clergy, of physicians, and even of men of science. The professional comic is the all-enveloping rut and the discerning layman is usually the stone that throws the wheel out on to new ground. This is the value of the jury. The juror is a layman but unfortunately he is too much a strangled and manacled servant to have either a layman's self-respecting freedom or the self-imposed constrictions of professionalism. He is never quite himself because he is under duress involuntarily doing work of professional nicety—involving complex calculation and insight to human motives, a gift of interpretation, a sense of probability, the elimination of personal bias. He is physiologist, pathologist, physicist, psychologist, detective, financier, moralist, jurist. And he is only a lackey without personality.

It is comparatively seldom that the witness who is sworn to give the "whole truth" is permitted to do so. The juror sees the effort of counsel to prevent his getting hold of it. He notes how the wooden "rules of evidence" sometimes cut out a body of testimony, the

pores of which are juicy with the very quintessence of evidence. The keen juror, scenting the aroma, longs to tap or squeeze out the sap of truth. But who is he, forsooth, but a juryman?

A physician, speaking out of much experience as an expert witness, told his medical associates that it is not enough that the evidence which they might be called on to give should be the truth. This, indeed, he said, has little to do with it for it must agree with the lawyers' and the court's idea of what is evidence, which is sometimes quite another thing.

Meantime the poor juror is between the devil and the deep sea—between the professional battle to suppress facts and his own sense of essential truth; between his own real conscience and the artificially imposed conscience of the court. So long, therefore, as "the evidence" is prescribed by the court the juror ought not to be sworn to render verdict according to "the evidence," but without prejudice to strive to be just to all parties concerned. No other discipline is just to any party concerned.

There is strong tendency to reduce the employment of juries. The question is whether there could not be a better kind of jury, partaking at once of lay and of professional advantages. It would be perhaps a permanent board adequately paid, non political, smaller than our present jury, not bound by the rule of unanimity, having large authority in taking and using testimony, and treated with the dignity due to the upper officers of a court of justice. In other words the disciplinary irritants, drawbacks, indignities, coercion, threats and sources of indifference and personal bias should be reduced to the minimum. Only thus can the balances of the juror's mind work as freely as possible in the valuation of evidence—which evidence, in very truth is not limited to the sworn statements of witnesses or other facts of the testimony but grows out of the interpretation of experience and of human nature in the large. The system, whatever it is, should not indifferently permit or encourage self-defeat. Justice to the contending parties rests in no small degree on justice to the jury.

ON THE PRODUCTION OF AN ARTIFICAL HISS.

By E. B. TITCHENER.

(*Read November 6, 1914.*)

In *Nature* of May 29, 1913, Lord Rayleigh asked to be told "how to make an artificial hiss." I replied that, if Köhler's observations are reliable, "a Galton whistle, set for a tone of 8,400 v. d., will give a pure *s*."[1] Lord Rayleigh, however, was not impressed by the suggestion.[2]

It occurred to me that the question might be put to the test of experiment. The sound of a Galton whistle set for 8,400 v. d. might be imitated by the mouth, and a series of observations might be taken upon material composed partly of the natural (mouth) sounds and partly of the artificial (whistle) tones. If a listening observer were unable to distinguish between the two stimuli, and if the mouth-sound were shown, phonetically, to be a true hiss, then it would be proved that the whistle also gives an *s*, and Lord Rayleigh would be answered.

The experiment was more troublesome that I had anticipated; but I may say at once that it has been carried out, and with affirmative result.

We used an Edelmann-Galton whistle (No. 423) actuated by a rubber bulb.[3] Our first difficulty was to find a competent experimenter. For the sound of the whistle is clean-cut, uniform, so to say dogmatic. This very definite stimulus has to be duplicated by a certain setting of tongue and lips, and by voluntary regulation and

[1] *Nature*, July 3, 1913; W. Köhler, *Zeits. f. Psych.*, LXIV., 1913, 93.

[2] *Nature*, July 31, 1913.

[3] The bulb that comes with the instrument must soon be renewed. It may be worth while to point out that bulbs of white or grey sulphur-coated rubber should never be employed; the fine dust chokes the mouthpiece and plays havoc, *e. g.*, with terminal determinations. We use a black rubber that is slightly more flaccid than that furnished by Edelmann.

direction of a current of air. But not only are there gross differences in the mode of formation of the *s*-sound; there are also individual differences, due apparently to the arrangement of the teeth, the shape and size of the tongue, and so forth;[4] and beside these, there are differences in ability to maintain the sound begun, to hold it from fluctuation during its course. The sound that one emits, on first trying to imitate the whistle, may therefore be almost comically wrong,—broad, harsh, irregular, soft, wavery, instead of sharp and keen.

We presently found two experimenters, Mr. N. P. Stephens and Mr. P. T. Carson, who after practice were able in a large proportion of successive attempts to reproduce the sound of the whistle. Neither these nor the other volunteers whom we tried out could, however, imitate the sound obtained by the ordinary vigorous squeeze of the bulb, such a squeeze as one gives in determinations of the terminus. We therefore had recourse to a compression which, though sharp and definite, was still weaker than that by which the whistle is usually actuated. The difference between the mouth-sound and the whistle-sound given at full intensity is, so far as our observations go, that the latter is beady, intense, compact, while the former is broader, weaker, coarser. The compression which we used was, nevertheless, strong enough to yield clear dust-figures by the Kundt method.[5] No doubt, it varied somewhat, both in the experimental series and in the dust-figure determinations. But the hand-pressure becomes rather surprisingly even, with a little practice; and determinations made at different times by Dr. Foster varied only between the limits of $8{,}594 \pm 63$ v. d. and $8{,}522 \pm 27$ v. d. It is therefore certain that we were working in the near neighborhood of Köhler's optimal 8,400 v. d.

[4] See, *e. g.*, C. L. Merkel, "Physiologie der menschlichen Sprache," 1866, 186 ff., Taf. III.; G. H. von Meyer, "The Organs of Speech," 1884, 314f.; G. E. Sievers, "Grundzüge der Phonetik," 1885, 56 ff., 122 f.; O. Jespersen, "The Articulations of Speech Sounds," etc., 1889, 61 f., 87; H. Hoffmann, "Die Lautwissenschaft," etc., 1901, 62; W. Vietor, "Elemente der Phonetik," etc., 1887, 125 ff.; E. Seelmann, "Die Aussprache des Latein," etc., 1885, 245 f.; H. Sweet, "A Handbook of Phonetics," 1877, 33, 39 f.

[5] M. T. Edelmann, "Studien über die Erzeugung sehr hoher Töne, etc.," Drude's *Annalen*, II. [CCCVII.], 1900, 469 ff.

An ideal experimental series would now have consisted of equal numbers of mouth-sounds and whistle-sounds arranged in haphazard order. But the experimenters could not thus accurately reproduce the whistle-sound. If the series chanced to call for several mouth-sounds in succession, then the setting of the mouth could be maintained; but if mouth and whistle alternated, or if a single mouth-sound were to be given among several whistle-sounds, there was need of readjustment and possibility of initial failure. It seemed best that whenever the mouth-sound was obviously wrong, and when (as sometimes happened) the squeeze of the bulb was too light or too heavy, the experimenter should say: "Don't count that! Repeat!" and should simply try again. This procedure gave the observers a certain advantage; but we thought it preferable to a voluntary change or a further haphazard determination of the stimulus. The more successful of our two experimenters, Mr. Stephens, was obliged, even at his highest level of practice, to repeat the mouth-sound in 12 to 15 per cent. of the trials.

There was a further complication. The observers sat at a distance of not more than 1 m. from the source of sound; they declared that, if they were to judge discriminatingly, they must be as near as possible; and they tended to lean or move in toward the experimenter. The preliminary experiments showed that, under these conditions, their judgment might be influenced by secondary indications—the direction of the sound, the noise of breathing, of setting the mouth, even of the squeeze of the bulb, the noise of preparatory movements in general. Hence they were informed before the regular series began that these indications were not to be relied upon, but that the experimenter might in any given test make misleading preparations. In fact, 50 per cent. of the mouth-sounds were accompanied by noises of bulb and table, and 75 per cent. of the whistle-sounds by noises of breathing and mouth-setting. The numerical results and the introspective reports prove that this ruse was successful. The observers based their discriminations, for the most part, upon the temporal course and the "size" of the stimuli; the sound was judged to be "whistle" if it was hard, clear-cut, abrupt, and to be "mouth" if it was fluctuating, "trembly," soft, diffuse. Sometimes pitch was referred to (whistle higher), and sometimes

intensity (whistle louder). The number of confusions testifies that these differences were not very dependable.

The first series of experiments (July, 1914; Mr. Stephens, experimenter; Miss F. A. Bean, observer) consisted, as planned, of 70 mouth-sounds and 70 whistle-sounds taken in haphazard order. Aside from the disturbances to which I have referred, the results were:

	Whistle.	Mouth.
Whistle judged as	60	10
Mouth judged as	25	45

Mr. Stephens was at this date relatively unpractised, while Miss Bean had had extended practice in the discrimination of whistle-tones. The number of confusions (25 per cent. of the whole number of observations) was, evidently, large enough to warrant a continuation of the experiment.

Other series, made by Messrs. Stephens and Carson with other observers, brought results of the same numerical order; they need not be cited. I pass at once to the two final series made (August, 1914) by Mr. Stephens. The first comprised two part-series of 50 tests, each composed of 25 mouth-sounds and 25 whistle-sounds. The observer, Dr. W. S. Foster, knew the plan of the investigation, had himself tried to reproduce the whistle-tone by mouth, and had had recent and unusually extended practice in the discrimination of whistle-tones. The percentages of confusion were:

Whistle judged as mouth	18
Mouth judged as whistle	20

or an average confusion of 19 per cent.

In the second series, two part-series of 50 tests were composed, the one of 22 whistle and 28 mouth sounds, the other of 28 whistle and 22 mouth sounds. The observers, Dr. E. G. Boring, Dr. L. D. Boring and Dr. M. E. Goudge, sat together for the experiment. Dr. E. G. Boring had had a good deal of practice with the whistle, and the other observers had performed the regular laboratory experiments in which it is employed. All three were, however, given special practice (with knowledge) in the discrimination of the stimuli now to be used. The percentages of confusion were:

Observer.	Whistle Judged as Mouth.	Mouth Judged as Whistle.
E. G. B.	20	36
L. D. B.	45	38
M. E. G.	35	39

or an average confusion of 35.5 per cent.

There is, naturally, a tendency, on the part of the practised observers, to judge "mouth as whistle" more often than "whistle as mouth": the percentages are, for Miss Bean, 35.7 : 14.3, and for Dr. E. G. Boring, 36 : 20. Dr. Foster, who can hardly be deceived, gives approximately equal percentages of confusion; but in his case too the ratio 20 : 18 favors "mouth as whistle." The less practised observers, however, offset each other. I had expected a far greater preponderance of correct judgments of the whistle, *i. e.*, a lesser number of judgments of "whistle as mouth"; and I think that the percentages actually obtained speak well for the skill of the experimenter.

It remains to show that our mouth-sound was a hiss. Neither of the experimenters was versed in phonetics; but we asked them to observe and describe carefully the position of lips and tongue during imitation of the whistle-sound. Mr. Stephens writes:

> "The position of the tongue is substantially, so far as I can judge, the same as that in which we produce the sound of the letter *s-s-s*. The sides of the tongue are so curled up that they rest against the inside of the upper teeth, on the sides. The middle of the tongue thus of course forms a hollow, up to the tip,—which very nearly touches the roof of the upper jaw just about a quarter-inch above the upper teeth . . . . For the production of a light hiss which is not to be heard loudly the tongue-muscles are semi-tightened as also are the muscles of the jaw and throat. The thin column of air which is forced lightly between the tip of the tongue and the point on the roof of the mouth makes production of sound. Teeth along sides and back are possibly 5 or 6 mm. apart, thus leaving plenty of opening for ejection of air. The lips stand a quarter-inch apart, with little or no drawing at the corners, for the light hiss. Lips, in the production of this sound, play little or no part; they merely are separated sufficiently so as not to interfere with air and sound. Unless they are well apart, however, they do interfere with the intensity and seeming pitch of the hiss."

This is a very fair amateur account of the production of a hiss; and if it is compared with the formula given, *e. g.*, by Jepersen, we

cannot doubt that Mr. Stephens was sounding an English *s*.[6] At the point where I have marked an omission, he draws a diagram which, with allowance made for amateur draughtmanship, is identical with the "[s] nach Bremer" of Jespersen's plate; it is needless to say that he had never opened Jespersen's book. Mr. Carson gives a very similar account, except that he appears to place the tip of his tongue a trifle further forward, and thus to approximate the German *s*. It is quite clear, then, that the experimenters were hissing.

So we have the artificial hiss that Lord Rayleigh asked for. It may be too weak for his purposes; and, more especially, it may be of too brief duration. We were able, however, to match the abrupt hiss of the experiments to a continuative hiss sounded by a second Edelmann whistle (No. 679) connected with the Whipple tanks: intensively, the match was only approximate; qualitatively, we regard it as fairly accurate.

For the qualitative determination we employed two methods. (1) The sound of whistle No. 423, actuated as in the experiments, was compared with three sounds from whistle No. 679 actuated by a regulated current of air from the tanks. These three sounds lay at what we supposed to be the point of equality with the sound of No. 423, at a pitch some 400 v. d. higher, and at a pitch some 400 v. d. lower. The three comparative pitches were intermixed in haphazard order; both time-orders were presented; and for the final series of observations we had the services of Professor H. P. Weld, a skilled musician as well as psychologist. (2) By a roundabout method of determination, which involved reliance on the Edelmann tables, we established the "identical" pitch of No. 679 as 8,430 v. d. Since Professor Weld's judgment made this pitch equal to or very slightly lower than the given pitch of No. 423, and since the error of our determination (provided always that Edelmann's tables are correct) can hardly have exceeded ± 100 v. d., we may assume that the two whistles gave very nearly the same *s*.

[6] O. Jespersen, "Lehrbuch der Phonetik," 1904, 34, 127 f. Mr. Stephens' use of the word "hiss" was spontaneous, not due to suggestion.

We were unable to determine the pitch of the continuative hiss by the Kundt dust-method; the lycopodium powder obstinately refused to move.

The accuracy of the Edelmann tables has been questioned by C. S. Myers ("On the Pitch of Galton Whistles," *Journ. of Physiol.*, XXVIII., 1902, 417 ff.). Edelmann does not give the m. v. of his scale readings; but it is possible that his technique is so accurate that the variation is minimal, or even that a single count suffices. Neither does he tell us how he compresses his bulb; it is probable that he uses some mechanical device which ensures a constant compression. We have ourselves made the following determinations with whistle No. 423 (temperature read as the mean of four thermometers):

(1) Ordinary vigorous squeeze, such as is employed in terminal determinations:

Found from 5 trials with the dust-method..... 8,897 ± 18 v. d.
By Edelmann's table..... 8,775

(2) Weaker squeeze, used in our experiments:

Found by dust-method..... 8,594 ± 63 v. d. to 8,522 ± 27 v. d.
By Edelmann's tables + judgments of coincidence of tones 8,430

(3) Very violent squeeze, 10 trials by dust-method..... 9,046 ± 71 v. d.

It is clear that Edelmann uses a "normal" compression, of the same order as that which an experimenter naturally employs for terminal tests. Random determinations of our two whistles at different points of the scale, with the ordinary vigorous squeeze, agree within about 100 v. d. with the Edelmann tables.

We have had but little experience with continuous tones under change of water-pressure; but we find, so far, that the pitch of our whistles does not alter appreciably within the limits of 90 to 160 mm. of pressure.

While, then, we do not question the accuracy of Myers' determinations, we think that there is no need either to doubt the reliability of the Edelmann whistle under "normal" conditions.

MINUTES

MINUTES.

Stated Meeting January 2, 1914.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

Invitations were received:

From La Reale Accademia de Ciencias y Artes de Barcelona to be represented at the celebration of the 150th anniversary of its foundation on the 18th, 19th and 20th of January next.

From the Regents of the University of the State of New York to be represented at the Inauguration of John Huston Finley as President of the University and Commissioner of Education of the State of New York, at Albany on January 2d, 1914.

The list of donations to the library was laid on the table and thanks were ordered for them.

The decease was announced of James MacAlister, A.M., LL.D., at sea on December 11, 1913; æt. 74.

Prof. E. G. Conklin read a paper on "Some Facts and Factors of Development"; which was discussed by Doctors Donaldson and Dercum, and Prof. Conklin.

The judges of the annual election of officers and councillors held on this day between the hours of two and five in the afternoon, reported that the following named members, according to the laws, regulations and ordinances of the Society, were elected to be the officers for the ensuing year:

President.

William W. Keen.

Vice-Presidents.

William B. Scott,
Albert A. Michelson,
Edward C. Pickering.

Secretaries.

I. Minis Hays,
Arthur W. Goodspeed,
Amos P. Brown,
Harry F. Keller.

Curators.

Charles L. Doolittle,
William P. Wilson,
Leslie W. Miller.

Treasurer.

Henry LaBarre Jayne.

Councillors.

(To serve for three years.)
Samuel Dickson,
Ernest W. Brown,
Morris Jastrow, Jr.,
Arthur Gordon Webster.

Stated Meeting February 6, 1914.

William W. Keen, M.D., LL.D., President, in the Chair.

Mr. Samuel Rea, a newly elected member, subscribed the Laws and was admitted into the Society.

The decease was announced of the following members:

At St. Petersburg on January 2/15, 1914, Théodose Tschernyscheff, Director of the Geological Survey of Russia; æt. 67.

At Philadelphia on January 4, 1914, S. Weir Mitchell, M.D., LL.D.; æt. 84.

At Plattsburgh, N. Y., on January 7, 1914, William D. Marks; æt. 65.

At Cambridge, Mass., on January 14, 1914, Benjamin Osgood Peirce, Ph.D.; æt. 60.

At London on January 24, 1914, Sir David Gill, K.C.B.; æt. 70.

At Philadelphia on February 1, 1914, Charles E. Dana, C.E., æt. 71.

Dr. P. B. Hawk read a paper on "The Relationship of Water to Certain Life Processes, and More Especially to Nutrition"; which was discussed by Dr. Brubaker, Prof. Kraemer, Dr. Dercum, Prof. Keller and Mr. Carson.

Stated Meeting March 6, 1914.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

Invitations were received:

From the Deutsche Shakespeare Gesellschaft to participate in the celebration of the 50th Anniversary of its founding on April 22 to 24, 1914.

From the Premier Congrès de Police Judiciare Internationale to participate in the Congress to be held at Monaco on April 14 to 20, 1914.

From the 19th International Congress of Americanists to be held at Washington October 5 to 10, 1914, to be represented thereat.

The decease was announced of:

Edwin J. Houston, Ph.D., at Philadelphia, on March 1, 1914; æt. 67.

Stuart Wood, Esq., at Philadelphia, on March 2, 1914; æt. 62.

Dr. Samuel W. Stratton read a paper on "Standards of Quality;" which was discussed by Dr. Donaldson, Dr. Keller, Mr. Day and Mr. F. Rawle.

Stated Meeting April 3, 1914.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

A letter was received from Senatore Giovanni Celoria expressing the grateful thanks of the Committee for 283.25 lire contributed by the members of this Society as a subscription to the Giovanni Schiaparelli Memorial.

The decease was announced of:

Edward S. Holden, A.M., LL.D., at West Point on March 16th, 1914; æt. 68.

Sir John Murray, K.C.B., Sc.D., LL.D., at Edinburgh on March 16th, 1914; æt. 73.

The following papers were read:

"Explorations in the Hudson Bay Regions with references to Unusual Topographic and Hydrographic Features and Mineral Deposits," by Mr. Ambrose E. Lehman; which was discussed by Mr. Willcox, Mr. Garrison and Mr. DuBois.

"On Psychology as the Behaviorist Views It," by E. B. Titchener, Ph.D., Sc.D., LL.D.

Stated General Meeting April 23, 24 and 25, 1914.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

Thursday, April 23—Opening Session.

The decease was announced of George William Hill, Sc.D., LL.D., at West Nyack, N. Y., on April 18, 1914; æt. 76.

The following papers were read:

"The Physical Cause of the Unsymmetrical Equilibrium of the Earth Between the Land and Water Hemispheres, with a Theorem on the Attraction of the Terrestrial Spheroid," by T. J. J. See, Ph.D., U. S. Naval Observatory, Mare Island, California.

"Some Observations on the Psychology of Juries and Jurors," by Patterson DuBois, Esq., of Philadelphia.

"Factors of Influence in the Origin and Circulation of the Cerebro-Spinal Fluid," by Charles H. Frazier, M.D., Professor of Clinical Surgery, University of Pennsylvania. Discussed by Dr. Crile.

"Aspects and Methods of the Study of the Mechanism of the Heart Beats," by Alfred E. Cohn, A.B., M.D., Associate in Medicine, Rockefeller Institute for Medical Resarch, New York. (Introduced by Dr. Keen.) Discussed by Dr. Keen.

"The Kinetic System," by George W. Crile, M.D., Professor of Clinical Surgery, Western Reserve University, Cleveland. Discussed by Dr. Keen and Dr. W. P. Wilson.

"The Hereditary Basis of Certain Emotional States," by Charles

B. Davenport, A.M., Ph.D., Director of Station for Experimental Evolution (Carnegie Institution), Cold Spring Harbor, New York.

"Syriac Socrates—A Study in Syrian Philosophy," by W. Romaine Newbold, Ph.D., Professor of Philosophy, University of Pennsylvania.

"The Evolution of Pine Barren Plants," by John W. Harshberger, Ph.D., Professor of Botany, University of Pennsylvania.

"Segretion of 'Unit Characters' in the Zygote of Oenothera with Twin and Triplet Hybrids in the First Generation," by George Francis Atkinson, Ph.D., Professor of Botany, Cornell University. Discussed by Prof. Bradley M. Davis and Prof Kraemer.

"The Vegetation of the Sargasso Sea," by William G. Farlow, Ph.D., LL.D., Professor of Cryptogamic Botany, Harvard University. Discussed by Prof. Harshberger, Prof. Atkinson, and Prof. Farlow.

"A New Type of Oak for America," by William Trelease, Sc.D., LL.D.

Friday, April 24.

Executive Session—9:30 o'clock.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

The proceedings of the officers and council were submitted and the list of nominations for membership recommended for election this year was reported.

The following resolution recommended by the officers and council was adopted:

That a Committee of eight members shall be appointed by the president at the first executive session of the general meeting in every year to nominate officers and councillors for the succeeding year. The president shall be ex-officio a member of this committee, but shall have no vote except in case of a tie. This committee shall report at the stated meeting on the first Friday in December and its

report shall be regarded as nominations duly made for the respective offices. But nothing herein contained shall be considered as restricting the right of any individual member to make nominations as provided in the Laws.

Members are invited to make to this Committee suggestions of appropriate nominations and it shall be its duty to give full consideration to the same.

The President thereupon appointed the following members to constitute the Committee: Dr. Hays, Prof. E. W. Brown, Prof. Trelease, Dr. R. S. Woodward, Hon. Charlemagne Tower, Dr. Donaldson, Prof. Gummere, Prof. Farlow.

Morning Session—9:35 o'clock.

ALBERT A. MICHELSON, Ph.D., Sc.D., LL.D., F.R.S.

Vice-President, in the Chair.

The following papers were read:

"Phase Changes Produced by High Pressures," by Percy W. Bridgman, Instructor in Physics in the Jefferson Physical Laboratory, Harvard University. (Introduced by Prof. Goodspeed.) Discussed by Professors Pickering, Hobbs, Bogert, Noyes and Michelson.

"The Influence of Atmospheric Pressure on the Forced Convection of Heat from Thin Electric Conducting Wires," by Arthur E. Kennelly, Sc.D., Professor of Electrical Engineering, Harvard University.

"Some New Tests of Quantum Theory and a Direct Determination of h," by Robert Andrews Millikan, A.M., Ph.D., Professor of Physics, University of Chicago. (Introduced by Prof. Goodspeed.) Discussed by Prof. Michelson.

Discussion of "A Kinetic Theory of Gravitation": (1) Gravitation is Due to Intrinsic Energy of the Ether; (2) Transmission of Gravitation Cannot be Instantaneous, by Charles F. Brush, Ph.D., Sc.D., LL.D., of Cleveland. Discussed by Prof. Nipher.

"Behavior of Metals and Other Substances Under Stress Near the Rupture Point," by A. A. Michelson, Ph.D., LL.D., Professor of Physics, University of Chicago. Discussed by Prof. E. W. Brown, Mr. Bridgman, and Mr. Brush.

"On Highly Radio-Active Solutions," by William Duane, A.M., Ph.D., Assistant Professor of Physics and Research Fellow of the Cancer Commission, Harvard University. (Introduced by Prof. Goodspeed.) Discussed by Mr. Brush and Dr. Donaldson.

"Some Further Considerations in the Development of the Electron Conception of Valence," by K. G. Falk, of the Harriman Research Laboratory, Roosevelt Hospital, New York. (Introduced by Prof. Bogert).

"The Valence of Nitrogen in Ammonium Salts," by William Albert Noyes, Ph.D., LL.D., Professor of Chemistry, University of Illinois, and R. S. Potter. (Introduced by Prof. H. C. Jones.)

"Determination of the True Atomic Weight of Radium," by Gustavus Hinrichs, of St. Louis.

Afternoon Session—2 o'clock.

EDWARD S. PICKERING, D.Sc., LL.D., F.R.S.,

Vice-President, in the Chair.

Dr. Cyrus Adler in presenting a portrait of the late Samuel Pierpont Langley, LL.D., a former Vice-President of the Society, spoke as follows:

On behalf of a number of members of the Society I have the honor to present a portrait of Samuel Pierpont Langley, a former member and Vice President of the American Philosophical Society.

Mr. Langley, the third Secretary of the Smithsonian Institution, was a man of national and international fame which rested primarily upon his epoch making researches in solar physics. All of the recognition which came to him was based upon his discoveries in physics and astronomy. But he was also a pioneer in another field, being the first distinguished man of science to devote himself to the

subject of aerial navigation, at a time when this was not considered within the realm of scientific study. The mere fact that a man of his reputation and position gave serious attention to this important subject lent it an impetus and standing which it would not have otherwise received for many years and therefore greatly advanced the development of this science and art. But he did more than give an impetus, for he not only discovered principles of prime importance in connection with aerodynamics, but was the first to produce a machine heavier than air, supported and propelled by its own engine and possessed of no extraneous or lifting power which actually made an independent flight.

The first flight of a heavier than air machine which ever occurred took place over the Potomac River on May 6, 1896 and was succeeded by numerous other flights of various models, which he built, all of the monoplane type.

He had received honorary degrees from Oxford and Cambridge Universities in England and from Harvard, Michigan, Princeton and Wisconsin in the United States. Medals were awarded him by the National Academy of Sciences, the Royal Society of London, the American Academy of Arts and Sciences, the Institute of France and the Astronomical Society of France and he was a member or correspondent of all of these and many other learned Societies including the Academia dei Lincei of Rome.

He had an especial affection for the American Philosophical Society, one of the few organizations in which he accepted an office.

In addition to these great achievements, Mr. Langley was a man of wide culture and of deep sympathy and insight. That I was permitted to enjoy his friendship was one of the most profoundly valued and touching experiences of my life.

[The donors are Messrs. Cyrus Adler, Carl Barus, L. A. Bauer, Alex. Graham Bell, John A. Brashear, George F. Edmunds, George E. Hale, David Jayne Hill, George Gray, T. C. Mendenhall, Charles E. Munroe, Edward L. Nichols, Richard Olney, Henry Fairfield Osborn, Edward C. Pickering, Raphael Pumpelly, Edward B. Rosa, Frank Schlesinger, Samuel W. Stratton, Mayer Sulzberger, Elihu Thomson, Otto H. Tittmann, Charles D. Walcott, Andrew D. White and Robert S. Woodward.]

VICE-PRESIDENT PICKERING in accepting the portrait, said:

My acquaintance with Samuel Pierpont Langley goes back to the winter of 1870, when we crossed the ocean together to observe the total eclipse of the sun, in Spain. We maintained an unbroken friendship until his death, nearly forty years later. As a young man he was enthusiastic, and full of hope for the future. When placed in charge of the observatory at Pittsburgh, he found that the smoke in the atmosphere rendered stellar observations difficult. He therefore selected the sun as his object for study, since the smoke by cutting off the heat, rendered the air more steady. A skillful draughtsman, his drawing of that complicated object, a large sunspot, is probably the best ever made. In accepting the position of secretary of the Smithsonian Institution, he stipulated that he should be enabled to continue his scientific investigations. This led to the establishment of the astrophysical observatory, which has continued and extended his work to the present time. He devoted many years to the construction and improvement of the bolometer, one of the most delicate devices for measuring heat, and a most difficult instrument to adjust and use. For many years, the question of artificial flight had an absorbing interest for him. His investigations were long and laborious, and finally he attained success with a small model. When constructing a larger instrument, his sensitiveness induced him to avoid publicity, thus greatly annoying those whose business it is to keep the public informed of the latest news. They had their revenge when a misplaced nail in his launching apparatus ruined his aeroplane on its trial trip, and the subsequent ridicule and criticism saddened his last days, and shortened his life. The success of aerial navigation is largely due to his work, which has only received the credit it deserves since his death.

Langley, by his devotion to the advancement of human knowledge, well deserves a place among those whose portraits adorn these walls, and in the name of the American Philosophical Society held at Philadelphia for Promoting Useful Knowledge, I accept this portrait and extend grateful acknowledgments to the donors.

The following papers were read:

"The Magnetic Phenomena of Sun-spots."

"The General Magnetic Field of the Sun." (Illustrated with lantern slides.) By George E. Hale, Ph.D., LL.D., F.R.S., Director of the Solar Observatory of the Carnegie Institution at Mt. Wilson, Cal.

"On the Colors of the Stars in the Cluster M 13," by Edward E. Barnard, Sc.D., LL.D., Astronomer of the Yerkes Observatory, Williams Bay, Wis.

"The Use of a Photographic Doublet in Cataloguing the Position of Stars," by Frank Schlesinger, M.A., Ph.D., Director of the Allegheny Observatory, Allegheny Pa. Discussed by Prof. Pickering.

"The Distribution in Space of 90 Eclipsing Stars," by Henry Norris Russell, Ph.D., Professor of Astronomy, Princeton University.

"The Eclipsing variable Stars ψ Orionis and 88 d Tauri," by Harlow Shapley, Ph.D., of Princeton University Observatory. (Introduced by Prof. H. N. Russell.)

"Some Features of The Moon's Motion and a Problem in Isostasy," by Ernest W. Brown, M.A., Sc.D., F.R.S., Professor of Mathematics, Yale University.

"The United States as a Factor in World Politics," by Leo S. Rowe, Ph.D., LL.D., Professor of Political Science, University of Pennsylvania.

"A Sumerian Nature Hymn from Nippur, of the Time of the Dynasty of Agade, 2800–2600 B. C.," by George A. Barton, Ph.D., Professor of Biblical Literature, Bryn Mawr College.

Evening Session—8:15 o'clock.

Arthur L. Day, Ph.D., Director of the Geophysical Laboratory of the Carnegie Institution of Washington, gave an illustrated lecture on "Some Observations of the Volcano Kilauea in Action."

Saturday, April 25.

Executive Session—9:30 o'clock.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

Prof. George F. Atkinson and Prof. Charles Edwin Bennett, recently elected members, subscribed the Laws and were admitted into the Society.

Pending nominations for membership were read and spoken to.

Mr. J. Edward Whitfield and Dr. James McKeen Cattell were appointed tellers of election and the Society proceeded to ballot for members.

The tellers of election reported that the following nominees had been elected to membership:

Residents of the United States.

Charles Greeley Abbot, S.B., Washington.
James Wilson Bright, Ph.D., LL.D., Litt.D., Baltimore.
Bradley Moore Davis, A.M., Ph.D., Philadelphia.
Thomas McCrae, A.B., M.D., Philadelphia.
William Diller Matthew, A.M., Ph.D., New York.
Alfred Goldsborough Mayer, Ph.D., M.E., Washington.
Samuel Jones Meltzer, M.D., LL.D., New York.
John Campbell Merriam, B.S., Ph.D., Berkeley, Cal.
Robert Andrews Millikan, A.M., Ph.D., Chicago.
William Albert Noyes, Ph.D., LL.D., Urbana, Ill.
Stewart Paton, M.D., Princeton.
Richard Mills Pearce, Jr., M.D., Philadelphia.
Palmer Chamberlaine Ricketts, C.E., LL.D., Troy.
Harold A. Wilson, M.A., D.Sc., F.R.S., Houston.
Frederick Eugene Wright, Ph.D., Washington.

Foreign Residents.

Shibasaburo Kitasato, M.D., Tokyo.
Heike Kamerlingh Onnes, Ph.D., Leyden.
Vito Volterra, Sc.D., Ph.D., Rome.

Morning Session—10 o'clock.

WILLIAM B. SCOTT, Sc.D., LL.D., Vice-President, in the Chair.

The following papers were read:

"Primary Cambrian Manganese Deposits of Newfoundland," by Nelson C. Dale, Fellow, Princeton University. (Introduced by Prof. W. B. Scott.)

"Geology of the Wabana Iron Ores of Newfoundland," by Albert O. Hayes, Fellow, Princeton University. (Introduced by Prof. W. B. Scott.)

"Hewettite, Metahewettite and Pascoite, Hydrous Calcium Vanadates," by W. F. Hillebrand, Ph.D., H. E. Merwin and Frederick E. Wright, Ph.D., of U. S. Geological Survey.

"The Relations of Isostasy to a Zone of Weakness—the Asthenosphere," by Joseph Barrell, Ph.D., Professor of Structural Geology, Yale University. (Introduced by Prof. Charles Schuchert.)

"Evidence for a Pulsational Change of Climate in the Libyan Desert," by William H. Hobbs, Ph.D., Professor of Geology, University of Michigan.

"The Cretaceous-Tertiary Boundary in the Rocky Mountain Region," by F. H. Knowlton, U. S. Geological Survey. (Introduced by Prof. John M. Clarke.) Discussed by Prof. W. B. Scott.

"The Geologic and Biologic Results of a Study of the Tertiary Floras of Southeastern North America," by Edward W. Berry, Associate Professor of Paleontology, Johns Hopkins University. (Introduced by Prof. William B. Clarke.) Discussed by Prof. W. B. Scott.

"On Multiple Treatment of One and the Same Story 'Motif,'" by Maurice Bloomfield, Ph.D., LL.D., Professor of Sanskrit, Johns Hopkins University.

"Some Biblical Miracles," by Paul Haupt, Ph.D., LL.D., Professor of Semitic Philology, Johns Hopkins University.

"The Sumerian Pronunciation of the Name 'Ninib' as the Chief Deity of Umma," by Alfred T. Clay, Ph.D., Laffan

Professor of Assyriology and Babylonian Literature, Yale University.

"Panama Tolls and Tonnage Rules," by Emory R. Johnson, Ph.D., Professor of Transportation and Commerce, University of Pennsylvania.

"Passamaquoddy Morphology," by J. Dyneley Prince, A.B., Ph.D., Professor of Semitic Languages, Columbia University, New York.

Afternoon Session—2 o'clock.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

Prof. William Albert Noyes, of Urbana, Ill., and Prof. Bradley Moore Davis, of Philadelphia, newly elected members, subscribed the Laws and were admitted into the Society.

Dr. WILLIAM G. FARLOW unveiled a Wedgwood medallion portrait of the late Sir Joseph Dalton Hooker, O.M., G.C.S.I., C.B. and spoke as follows:

Today we are so fortunate as to be able to add a medallion of one of the world's great botanists to the already large number of memorials of distinguished men which adorn this Hall and give it a dignity which is justly envied by other scientific societies in this country. Joseph Dalton Hooker, the more distinguished son of a distinguished father, was born in Halesworth, Suffolk, England, in 1817 and, retaining his scientific activity until the last, died at Sunningdale in 1911, a record very rarely equalled.

When four years of age his father, Sir William Hooker, removed to Glasgow, where he had been appointed Professor of Botany in the University, so that from his early childhood, the son was placed in surroundings which naturally pointed to botany as his life work. While a student of medicine, Hooker had the opportunity of reading the proofs of Darwin's "Voyage of the Beagle" which aroused in him an intense desire to travel. This desire was fortunately soon gratified, for immediately after receiving the degree of Doctor of Medicine in 1839, he was appointed assistant surgeon and botanist to the *Erebus* under the command of Sir James Ross, then about to start on his memorable voyage to the Antarctic regions.

On his return in 1843 Hooker made his home at Kew where his father had been appointed director of the Royal Gardens. He was appointed assistant director in 1855 and, on the death of his father in 1865, director, which position he held till his retirement twenty years later. We always think of Hooker as at Kew. It was there, aided by the large collections formed in great part by his father and himself, that he finished his different floras; there that he brought to perfection the Garden which had been raised by his father from insignificance to be the leading botanical garden of the world; there that many American botanists were received with a cordiality doubly welcome because they were encouraged by his sympathy and aided by his advice.

Hooker was undoubtedly the leading botanical systematist of his day. For this branch of botany he not only had great natural ability, but he also had opportunities for studying in the field the floras of distant and little-explored regions such as few trained botanists have had. Besides his Antarctic voyage, when he visited New Zealand and Tasmania as well as more southern regions, he spent the years 1848 to 1851 in an exploration of the Himalayas in company with Thomas Thomson,—an expedition involving great hardships among hostile people, but rich in results, and later he made trips to Palestine and Morocco. On his last long journey in 1877, he travelled with his old friend, Asa Gray, among the Rocky Mountains and in California.

On this occasion we need not consider in detail Hooker's various descriptive works on the floras of the countries he had visited, nor works like the great "Genera Plantarum," written in collaboration with Bentham, technical systematic treatises belonging to the classics of botany. Let us recall rather those qualities of Hooker which made him more than a systematist, which entitled him to rank with Darwin, Wallace, Lyall and Huxley in the brilliant group of naturalists which has never been surpassed, if it has ever been equalled, in any other country. Like Darwin, Hooker began his botanical career as an explorer of remote regions. The delightful account of the "Voyage of the Beagle" has its counterpart in the "Himalayan Journals" of Hooker. In both we recognize the fact that the

authors were something more than interesting relators of what they had seen. To them biological facts were only significant as indicating so many steps in the sequence of cause and effect. The genius of Darwin was manifested in his ability to see clearly in the beginning of his career the true direction in which the facts he had observed pointed, so that his lifework was unified, one step leading inevitably to another in the development of a great theory. It was much the same with Hooker. The writings which mark him as a philosophical botanist are the "Introductory Essay to the Flora of Tasmania"; the "Essay on the Distribution of Arctic Plants"; the "Discourse on Insular Floras"; the address at York on "Geographical Distribution," and the "Essay on the Vegetation of India," publications extending over a period of forty years.

The study of plant distribution involving a consideration of the geological phenomena which could account for it, and also of the question as to the effect of altered environment in modifying the characters of plants, naturally led to the fundamental question: What are Species and what are Varieties? One who, like Hooker, was master of the facts and without prejudice, could not fail to recognize that species are not fixed creations, but transitional stages in the progress of evolution. Hooker was a Darwinian even before the appearance of the "Origin of Species." It has been said with truth that, with the exception of Wallace, Hooker was the first adherent of Darwin in his views on evolution. How much that means is hardly realized at the present day. With us the question is not whether there is such a thing as evolution in plants and animals. We accept evolution as a fact, and, if there be any question, it is as to whether the explanation of the mode of its operation as presented by Darwin was satisfactory in all its details. In 1859, however, the date of the publication of the "Origin of Species," and for a considerable number of years later it required a good deal of courage as well as an unbiased mind for anyone, especially for an Englishman, to declare his assent to the revolutionary views advanced by Darwin.

In closing I may be permitted to repeat the words describing the position of Hooker among botanists on the occasion of the presentation to him of the Linnean gold medal at Stockholm in 1907.

"By scientific expeditions to many different parts of the world he has revealed the secrets of their vegetation. His extraordinary experience embraces both the nature of tropical India as also the subtropical and temperate climates, as well as of the cold antarctic regions. The contents of his floristic works are therefore exceedingly rich. He has furthermore enriched botany by splendid works in other departments of this science, for instance concerning the geographical distribution of plants, their classification and other matters."

I have the honor of unveiling the medallion of Sir Joseph Dalton Hooker, the energetic explorer, the eminent systematist, the distinguished investigator of the problem of plant evolution.

The following papers were read:

"The Burgess Shale Fauna of the Canadian Rockies," by Charles D. Walcott, Ph.D., Sc.D., LL.D., Secretary of the Smithsonian Institution, Washington.

"Summary of Researches, Department of Terrestrial Magnetism, 1904–14." (Illustrated.) By Louis A. Bauer, Ph.D., D.Sc., Director of the Department of Terrestrial Magnetism of the Carnegie Institution, Washington, D. C. Discussed by Prof. Pickering.

Symposium on Physics and Chemistry of Protoplasm:

"The Germ Plasm as a Stereochemic System," by Edward T. Reichert, M.D., Prof. Physiology in Univ. of Penna. (Introduced by Dr. Keen.)

"Arrangement and Distribution of Substances in the Cell," by Edwin Grant Conklin, Ph.D., Sc.D., Professor of Zoology at Princeton University.

"Vital Staining of Protoplasm," by Herbert McLean Evans, M.D., Associate Professor of Anatomy, Johns Hopkins University. (Introduced by Prof. Piersol.)

"The Physical State of Protoplasm," by G. L. Kite, M.D., Ph.D., Phipps Institute, Philadelphia. (Introduced by Prof. McClung.)

"The Physico-Chemical Organization of the Cell," by Lawrence J. Henderson, A.B., M.D., Assistant Professor of Biological Chemistry, Harvard University. (Introduced by Dr. H. F. Keller.)

Stated Meeting May 1, 1914.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

Dr. Thomas McCrae, of Philadelphia, a newly elected member, subscribed the Laws and was admitted into the Society.

Letters accepting membership were received from:

Charles Greeley Abbot, S.B., Washington.
Bradley Moore Davis, A.M., Ph.D., Philadelphia.
Thomas McCrae, A.B., M.D., Philadelphia.
Samuel James Meltzer, M.D., LL.D., New York.
William Albert Noyes, Ph.D., LL.D., Urbana, Ill.
Stewart Paton, M.D., Princeton.
Richard Mills Pearce, Jr., M.D., Philadelphia.
Palmer Chamberlaine Ricketts, C.E., LL.D., Troy.
Frederick Eugene Wright, Ph.D., Washington.

The list of donations to the library was laid upon the table and thanks were ordered for them.

The decease was announced of George F. Baer, at Philadelphia, on April 26, 1914, in the 72d year of his age.

Prof. J. Russell Smith read a paper on "Tree Breeding with Relation to Conservation and the Food Supply," which was discussed by Prof. Bradley M. Davis and Dr. Keen.

Hon. Charlemagne Tower, Chairman, presented and read at length the report of the Committee on the Date of Origin of the Society.

On motion, by unanimous vote, the report was accepted; the year 1727 was declared to be the date of foundation of the Society, in accordance with the finding of the committee; and the committee was discharged with the thanks of the Society for its exhaustive report.

Stated Meeting October 2, 1914.

WILLIAM W. KEEN, M.D., LL.D., President in the Chair.

Dr. Richard Mills Pearce, a newly elected member, subscribed the Laws and was admitted into the Society.

Letters accepting membership were received from:

James Wilson Bright, Ph.D., LL.D., Litt.D., Baltimore.

William Diller Matthew, A.M., Ph.D., New York.
Alfred Goldsborough Mayer, Ph.D., M.E., Washington.
John Campbell Merriam, B.S., Ph.D., Berkeley.
Robert Andrews Millikan, A.M., Ph.D., Chicago.
Harold A. Wilson, M.A., D.Sc., F.R.S., Houston.
Shibasaburo Kitasato, M.D., Tokyo.
Heike Kamerlingh Onnes, Ph.D., Leyden.
Vito Volterra, Sc.D., Ph.D., Rome.

Invitations were received:

From the Ohio State Archaeological and Historical Society, to be represented at the dedication of its Museum and Library Building, at Columbus, on May 30th.

From the University of Missouri, to be represented at the 75th Anniversary of its founding on June 3d.

The decease was announced of the following members:

Edward Suess, Ph.D., at Vienna, on April 29, 1914, æt. 73.

William Aldis Wright, LL.D., D.C.L., Litt.D., at London, on May 19, 1914.

John Robert Sittlington Sterrett, Ph.D., LL.D., at Ithaca, N. Y., on June 16, 1914, æt. 63.

Frederick William True, M.S., LL.D., at Washington, on June 25, 1914, æt. 56.

John Barnard Pearse, at Roxbury, Mass., on August 24, 1914, æt. 72.

Morris Longstreth, A.M., M.D., at Barcelona, Spain, on Sept. 19, 1914, æt. 68.

Special Meeting October 30, 1914.

William W. Keen, M.D., LL.D., President in the Chair.

A paper was read on "A new Means of Studying Submarine Animal and Vegetable Life," illustrated by moving pictures of tropical deep sea flora and fauna.

Stated Meeting November 6, 1914.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

The decease of the following members was announced:

Léon de Rosny, at Fontenay-aux-Roses (Seine), on August 28, 1914, æt. 78.

Theodore Nicholas Gill, M.D., Ph.D., at Washington, on September 25, 1914, æt. 77.

The following papers were presented:

"On Wireless Longitude Determination," by Eric Doolittle. Discussed by Professor C. L. Doolittle, Professor Snyder, Mr. Mitchell and Professor Miller.

"On the Production of an Artificial Hiss," by E. B. Titchener, Ph.D., D.Sc., LL.D.

Stated Meeting December 4th, 1914.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

An invitation was received from the Chairman of the 11th Annual Conference of Historical Societies to be held in Chicago in connection with and as part of the Thirtieth Annual Meeting of the American Historical Association, December 28 to 31, to participate in the conference by appointing delegates.

The decease was announced of

August Weismann, D.Ph., D.C.L., at Freiburg on November 5, 1914, æt. 80.

Charles Sedgwick Minot, M.D., Sc.D., LL.D., at Boston on November 19, 1914. æt. 61.

Alfred Thayer Mahan, LL.D., D.C.L., Rear Admiral U.S.N, at Washington on December 1, 1914. æt. 74.

Dr. John Mason Clarke of Albany read a paper on "The Magdalen Islands—a Relict Archipelago," which was discussed by Mr. Willcox and Prof. Pilsbry.

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